ON

THE PRESENT STATE

OF

THERAPEUTICS.

WITH SOME SUGGESTIONS FOR PLACING IT UPON A MORE SCIENTIFIC BASIS.

BY

JAMES ROGERS, M.D.,

FORMERLY PHYSICIAN TO THE BRITISH LEGATION AND TO THE ABOUCHOFF HOSPITAL AT ST. PETERSBURG.

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PREFACE.

AT the commencement of my professional career, I was much struck with the reported success of homœopathic treatment. I knew from satisfactory evidence that many remarkable recoveries had taken place under that method; but, like many other physicians who had no faith in the curative action of infinitesimal doses of medicine, I did not believe they could be ascribed to the action of the drugs employed. It appeared to me, however, that properly detailed histories of cases, in which homœopathic treatment had been employed, would afford us invaluable information with regard to what could be accomplished in disease by the *vis medicatrix naturæ.*

Since that period, some trustworthy reports have been furnished by physicians attached to homœopathic hospitals, which leave no reasonable doubt about the large proportion of recoveries that occur in their practice. From an examination of these reports in the second part of this work, it will be found that some homœopathic hospital practice gives results as satisfactory as those of any other method.

It is evident that the recoveries referred to in these reports must be ascribed either to the curative power of the organism itself, or to that aided by the action of the drugs. I shall endeavour to prove that they cannot be accounted for on the latter supposition. To do so, it will only be necessary to show, by complete and accurate calculations, that from the small quantities of medicine contained in homœopathic doses, they must be regarded, according to the known laws of matter, as quite inert: no explanation hitherto given of their supposed action affords the slightest support to the contrary opinion. Such calculations have already been made; and as there is no doubt of their accuracy, I shall freely make use of them.

The conclusion arrived at by *à priori* reasoning will be strengthened by showing, as well as my limited data allow me, that homœopathic and expectant treatment give nearly the same results. We shall thus have reason to conclude that the recoveries were effected by the natural resources of the organism, and that the histories of cases treated homœopathically may, therefore, be considered as so many illustrations of the natural course of disease.

As I have observed, however, in one or two recent works on therapeutics, many statements made respecting the action of drugs, which homœopathists claim as their discoveries, and which, if true, would go far in supporting the fundamental principle of their doctrine, I have thought it advisable on the present occasion to examine more closely than has yet been done upon what basis that principle is founded.

On reading over the descriptions of the properties of different drugs, such as aconite, arsenic, camphor, ipecacuanha, iron, mercury, etc., given in Dr. Ringer's work on Therapeutics, any one acquainted with homœopathic literature will perceive that many of his statements have been hitherto found only in works on homœopathy. If they be true, their source would be a matter of little importance; but I believe some of them are quite erroneous, such as that respecting the action of mercury: 'for it is singular how similar are the phenomena produced by mercury to those which result from syphilis.' At all events, I have arrived at a different conclusion from an examination of the properties of that drug. Under such circumstances, medical men will not probably consider my examination of the fundamental principle of homœopathy either out of place or unseasonable.

It may be said that many able writers have already shown the worthlessness of the homœopathic doctrine. I readily admit that it has been repeatedly and satisfactorily proved that most of its secondary principles are quite unfounded; but it by no means follows that the fundamental principle itself, *similia similibus*, must likewise be false. It appears to me that no author, with the exception of Dr. Jörg, has fairly grappled with that proposition, and shown in the only way in which its truth or falsity can be proved—by trials with drugs on healthy individuals—that it is not supported by facts.

A knowledge of the natural course of disease is of such immense importance to treatment, that every conscientious physician who takes an interest in the improvement of medicine ought to throw aside whatever prejudices he may have formed against homœopathy, and examine calmly and dispassionately the question of the comparative results of expectant and homœopathic treatment, the solution of which will undoubtedly form an epoch in therapeutics ; and he may probably find, as I myself have done, that the large proportion of recoveries which take place under homœopathic treatment may be a fact, although the principles of the doctrine are unfounded. I have had many opportunities for observing what occurs in homœopathic practice, both in hospitals and in dispensatories; and what I have there seen, explains in a natural and satisfactory manner why homœopathists adhere so firmly to their fallacious opinions. They ascribe the recoveries to the action of their drugs, and thus commit a mistake with which medical men of the old school are sufficiently familiar—*post hoc, propter hoc.*

The authority of Andral may perhaps have some influence on those physicians whose prejudices against homœopathy are so great, that they would otherwise look askance on any investigation at all connected with that subject. I may remark that the article, of which I shall give an extract, was published two or three years after he had made his celebrated but abortive trial of homœopathic treatment in the Hospital of La Charité. He says: 'Sans prejuger la question que les homœopathes ont soulevée dans ces derniers temps, sur la proprieté qu'auraient les agents curatifs de determiner dans l'organisme les maladies, qu'en allopathie on se propose de combattre par eux, nous croyons que c'est là une vue qu'appuient quelques faits incontestables, et qui à cause des consequences immenses qui peuvent en resulter, mérite au moins l'attention des observateurs. A supposer, ce qui est très probable, que Hahnemann soit tombé à cet égard dans l'exagération si facile aux theoriciens, parmi les faits nombreux, qu'il cite à l'appui de ses opinions, il est certain qu'il en est quelques uns qui sont parfaitement en harmonie avec sa pensée. Que l'on repète ses experiences, il est vraisembable que l'on en verra surgir quelques autres faits aussi authentiques. Qu'un esprit vigoureux médite ces faits; qu'il les compare après les avoir explorés sous toutes leurs faces; qui sait les consequences qui en pourraient jaillir!'

Some medical men object to the words allopathy and allopathic; but as common use has now rendered them convenient distinctive terms for expressing what would otherwise require the use of a phrase, I have not hesitated to employ them. In the latter half of my work I have frequently made use of the term non-homœopathic, as it involves no theoretical notions, and can be appropriately applied to such methods of treatment as the hydropathic and expectant, which could not with propriety be called allopathic, or treatment of the old school.

In attempting, however imperfectly, to call the attention of medical men to the present state of therapeutics, I have frankly expressed my opinions, and, I trust, have fairly appreciated those which happen to be opposed to mine. Opinions naturally appear to us more erroneous in proportion as they differ from those that we hold for the time; but whatever may be thought of my views, there can be little doubt that the great end and aim of all medical teaching—the improvement of therapeutics, can only be promoted by a calm, close, and impartial examination of that important subject.

CONTENTS.

PART I.

INTRODUCTION.

PAGE

CHAPTER II.

SECTION I.—NON-HOMŒOPATHIC TREATMENT OF ACUTE ARTICULAR RHEUMATISM.

SECTION II.—HOMŒOPATHIC TREATMENT OF ACUTE ARTICULAR RHEUMATISM.

CHAPTER III.

SECTION I.—NON-HOMŒOPATHIC TREATMENT OF INTERMITTENT FEVER.

SECTION II.—HOMŒOPATHIC TREATMENT OF INTERMITTENT FEVER.

CHAPTER IV.

SECTION I.—NON-HOMŒOPATHIC TREATMENT OF TYPHUS FEVER.

SECTION II.—HOMŒOPATHIC TREATMENT OF TYPHUS FEVER.

CHAPTER VII.

GENERAL MORTALITY IN SOME HOMŒOPATHIC AND NON-HOMŒOPATHIC HOSPITALS—SUMMARY OF THE COMPARATIVE RESULTS OF THE TREATMENT OF PARTICULAR DISEASES.

PART III.

CHAPTER I.

ON THE AID THAT PHYSIOLOGY, PATHOLOGY, AND CHEMISTRY CAN GIVE TO THERAPEUTICS.

PAGE

CHAPTER II.

ON THE NATURAL COURSE OF DISEASE.

CHAPTER III.

ON THE NECESSITY FOR PROVING DRUGS.

PAGE

CHAPTER IV.

ON THE IMPORTANCE OF ATTENDING SYSTEMATICALLY TO THE STATE OF THE PATIENT'S MIND IN THE TREATMENT OF DISEASE.

PART I.

INTRODUCTION.

THE ABSENCE OF FIXED PRINCIPLES IN THERAPEUTICS—STATISTICS.

AT first sight, it would seem to be an easy matter to settle the question of the efficacy of medical treatment. The medicines employed, as well as the progress of the disease after their administration, are known; but unfortunately it must be admitted, that to appreciate correctly the value of any plan of treatment, is one of the most difficult problems of practical medicine. The history of our art is but one long proof, so to speak, of the truth of this statement; and I may say that even at the present day, although the difficulties of such investigations have been lessened, every work on medical practice amply shows, that as yet there is nothing fixed or stable in therapeutics.

What is the cause of this absence of fixed principles in therapeutics? It evidently arises from our erroneous opinions respecting the action of drugs—from ascribing to them properties which they do not possess. It would be of the utmost importance to be able to ascertain accurately the causes that have led to the adoption of these erroneous views. They are to be attributed partly, no doubt, to the inherent difficulties of pathological research; but far more, I think, to the defective and injudicious methods of investigation which medical men have employed, and especially to their having overlooked one or two important points, of which it is necessary to have a clear idea, in order to be able to estimate accurately the influence of treatment on disease. I shall briefly advert to some of these causes.

It is evident that, in proportion as the diagnosis of disease was less understood, the possibility of confounding one diseased state with another must have been greater; and consequently, that of attributing the cure of one disorder, instead of another, to the action of a given medicine. As it became better understood, however, fewer mistakes of that kind would occur; and during the present century, this part of medicine has made such progress, that the chances of error from such a cause, as far as regards ordinary complaints, have been very much lessened, although considerable obscurity still overhangs the diagnosis of those of the nervous system.

The next cause which I shall notice is one that has had much greater influence than the former in leading us to adopt erroneous notions respecting the cure of disease. I mean our ignorance of the real properties of medicines. Even at the present day, our Materia Medica is in a state of chaotic confusion; and its history is merely a summary of the fruitless efforts of thousands of gifted men, searching as it were in the dark for the curative powers of drugs. Whilst we protest against the falsity of Hahnemann's unaccountable statement, viz. that as far as his knowledge goes, he is the first person who tried to ascertain the action of drugs by trials on healthy individuals, we must admit that homœopathy has hitherto given us the greater portion of our well-grounded knowledge of the properties of medicines. This, limited though it be, has been sufficient to show the inaccuracy of many opinions held, even in recent times, on the action of different articles of the Materia Medica. With such scanty knowledge as we possess of the action of drugs on healthy individuals, the difficulty of appreciating their action in disease, especially when several of them are administered in combination, becomes almost insurmountable; and, as an inevitable result, errors without number are added to the immense category of *post hoc, propter hoc.*

But by far the most abundant source of error in therapeutics, and one that is scarcely less so at present than it was centuries ago, is our ignorance of the natural course of disease. The earliest cases of recovery must have been those in which disease disappeared without treatment; and such cases must have frequently presented themselves to the notice of the first inhabitants of the earth. In the earliest medical writings extant, that law of the system—the tendency of diseased states to disappear of themselves, of diseased parts to return to the healthy state without the use of medicine—is distinctly admitted, and frequently referred to. Its existence has been recognised at every subsequent period of the history of medicine, and it has been adopted as the basis of many plans of treatment in ancient, as well as in modern times. It is true, the simple law has not been recognised and accepted as the expression of the phenomena observed in diseased states of the body. Their spontaneous disappearance has always been ascribed to some inscrutable power or entity, now called Nature, now Archeus, now Anima, Vital Force, etc.

But although it has always been admitted as a fact in therapeutics, that many cases of disease of a lighter kind, and even some of a severe and formidable nature, might get well without the use of medicine, it is only since the middle of the present century that the most important question ever asked in practical medicine, What can nature, and what can art, do in the cure of disease? has been examined in a more scientific manner.

To the Austrian school of medicine belongs the merit of having taken the first steps to ascertain the natural progress, or, as it has been called, the natural history of disease, on a large scale. I believe, however, that the results of homœopathic practice in the Gumpendorf Hospital and elsewhere first suggested the safety of trials, of which the importance was self-evident, to determine what the unaided resources of the system could do in some of its most formidable diseases, as pneumonia, typhus fever, etc. Medical men have not yet made such progress in that investigation, as its importance warranted us to expect. As I have already said, I consider it to be one of the most important that has ever been undertaken in practical medicine: it is even more important than the examination of the properties of drugs; for until we know the natural course of disease, it will be impossible to answer the question, What influence have drugs on it?

To estimate the value of a remedy would be a sufficiently simple and easy matter, if we could consider every recovery as a cure; but the fact, that a great number of cases of disease get well without the use of drugs, shows clearly that we cannot venture to draw that conclusion. As I have already stated, our knowledge of the action of drugs is so very limited, and our knowledge of the natural course of disease is so very incomplete, that at present it is almost impossible to determine precisely, in any case of recovery in which medicine has been used, what nature has done, or how much medicine has accomplished. Before we can pretend to estimate the influence of treatment on disease, it is necessary that we should know beforehand what diseased states may get well of themselves, as well as in what manner and in what time they may do so. In short, without a fixed standard of comparison, we cannot say in any case whether recovery has taken place by the unaided operations of the living body, or in consequence of the modifying influence of some medicine that was employed.

Although in the present state of our therapeutic knowledge, we cannot speak with much confidence of the action of medicine on disease in general, yet, by a proper use of means at our disposal, we may make some approach towards a solution of the question of the comparative influence of different plans of treatment in a given disease; and with so much the greater certainty, the larger the number of the cases observed. Thus, if we divide into two groups a large number of cases of any disease, selected as nearly as possible in the same general condition, in order to test the efficacy of two different plans of treatment, the natural tendency of the diseased states to disappear will be nearly equal in the two groups, and will not consequently affect in any important way the comparative results of the two plans of treatment; so that the difference between them can be fairly ascribed to the only manifest modifying cause—the action of the curative agents employed.

Up to a recent period of medicine, the only form of statistics employed by medical men consisted simply, to use a common phrase, 'in counting their cases;' and even at the present day, it is probably the form most generally employed by practitioners. If we ask a medical man why he prefers any particular method of treatment in any given disease, the answer is, that he has found that method more successful than any other; or, in other words, in a given number of cases of any disease, he has found that a larger number of recoveries took place under that method of treatment than under any other. It generally happens, however, that the phrase 'greater number of recoveries' is a very vague one, and often simply expresses an unknown number.

As our physiological and pathological knowledge became more accurate and extensive, it was seen that in many cases the use of the simple statistic formula of former times could only lead to erroneous conclusions; for it placed in one category cases of the same disease, to use the common phraseology, which often differed from one another by the absence or presence of some important condition, which of itself had a considerable influence on the mortality.

Several talented physicians, M. Louis and others, have attempted to give a greater extension to the employment of statistics in medicine, by dividing similar diseased states into groups or categories, corresponding to certain conditions which are admitted to exercise an influence on their progress, such as age, sex, habits, climate, hygienic condition, etc.; but I think it must be admitted that, as far as regards therapeutics, the labours of the numerical school have not completely realized what many physicians unreasonably expected from them. Statistics certainly give a precise and definite expression to facts which they otherwise would not have; but they cannot discover or explain them, nor ensure us from error in grouping them; they cannot elucidate the connection between the various phenomena of disease, especially the important one of cause and effect. Their value depends entirely on the accuracy with which the facts have been observed and classed: they are simply facts numbered and classed. In one form or another, however, they are indispensable in estimating the influence of treatment.

An able author has lately attempted to show that 30,000 cases of pneumonia, with a limited number of symptoms, would require to be collected before exactly the same combination of symptoms could be found in two cases. I do not object to the results of the mathematical formula; but it appears to me, that in the present state of pathology, the symptoms of disease give far too uncertain data to admit of the application to them of a mathematical formula with its rigid precision. Besides, the author to whom I have alluded seems to have made an assumption unwarranted in the present state of medical knowledge—that every variety in the combination of symptoms would necessitate a corresponding modification of the treatment. I believe he would be much embarrassed to say whether or not these modifications would be necessary to the success of the treatment; or, if necessary, on what principle they should be made. The best possible modification of the treatment of pneumonia would be to limit as much as possible the use of all energetic remedies; as the results of the expectant, and of Dr. Hughes Bennett's restorative, treatment of that disease have shown these methods to be far more favourable to recovery than the active practice of those practitioners, whose consciences would not allow them, 'with the principles of medicine before their eycs,' to be almost passive spectators of the progress of the disease.

As I have already said, the only legitimate use of statistics in therapeutics is to give a brief and precise expression to previously established facts. By presenting a summary of them at once to the eye, they may facilitate our endeavours to discover the relations that exist among them; but they cannot assist us in observing them, nor do they aid us much in judging of them. Still, statistics properly applied are essential to the progress of therapeutics.

CHAPTER I.

'SIMILIA SIMILIBUS CURANTUR,' ADOPTED AS THE THERAPEUTIC PRINCIPLE OF THE HOMŒOPATHIC SYSTEM—ITS APPLICATION IN PRACTICE QUITE ARBITRARY — MEANING OF SOME WORDS DEFINED.

WHEN Hahnemann was engaged in translating Cullen's work on the Materia Medica into German, his attention was arrested by Cullen's attempt to explain the action of cinchona bark. Cullen's explanation appeared unsatisfactory to him; and he determined to try what effects the drug would produce on himself, in order to see if in that way, perchance, he could find a clue that would enable him to explain its action. After using a large dose of that drug for several days, he had, on two successive days, an attack very similar to that of intermittent fever, which he thus describes: [1]—'The feet and points of the fingers, etc., became cold; then the heart began to throb; the pulse was small and quick; insupportable anxiety; trembling, but no chilliness; general prostration; then beating in the head, flushing of the cheeks; in short, all the usual symptoms of intermittent fever appeared one after the other, but without real febrile chilliness. I likewise remarked the usual particularly characteristic symptoms of intermittent fever—dulness of the senses, a sort of stiffness in all the joints, particularly the disagreeable benumbed feeling that seemed to have its seat in the periosteum of all the bones. The paroxysm lasted two to three hours, and was renewed only when I renewed the dose; otherwise not.' He was naturally struck by the result of his experiment; and it occurred to him that there was something more than an accidental relation between the power of curing disease, and that of producing in the healthy

[1] Cullen's *Mat. Med.* vol. ii. pp. 108–9, note. Translated into German by Hahnemann.

body symptoms similar to those of the disease cured.[1] 'If I am not deceived, such is really the case; otherwise, how was it that those violent tertian and quotidian fevers which I completely cured four and six weeks ago, without knowing how the cure was effected, by means of a few drops of cinchona tincture, should present almost the same array of symptoms which I observed in myself yesterday and to-day, after gradually taking, while in perfect health, four drachms of good cinchona bark by way of experiment?'

He repeated similar experiments on himself and other individuals with a similar result—that of producing the symptoms of intermittent fever by the use of cinchona bark. He naturally extended his experiments, by testing the effects produced in healthy individuals by other medicines, whose curative action in certain diseases had been well established. In every case he thought he found his conjecture confirmed, that drugs satisfactorily cured diseases similar to those produced by their action on healthy individuals.

. *Similia similibus* became the basis of his therapeutics; and although the principle was not a new one, as it had been frequently referred to by medical men since the time of Hippocrates, yet it must be confessed that in his hands it received a far greater, and in some respects more scientific, development, than any of its former supporters (even Stahl the Dane) had ever attempted to give it. The seeming simplicity and completeness of the principle are admirable. If true, it contains within itself a complete system of therapeutics: to find a remedy for any given case of disease, it is only necessary to discover a drug that can produce in the healthy individual symptoms similar to those of the disease to be cured.

Before attempting to examine more closely the fundamental principle of homœopathy, we must previously ascertain, if possible, what is understood by the relative term 'like.' Hahnemann has not formally explained the meaning which he attached to the word; but from what he says in two or three parts of his essay, *The Medicine of Experience*, I think we shall be able to

[1] A somewhat similar idea occurred to Störck of Vienna. Speaking of the action of stramonium, he asks, Might not this medicine be useful in the cure of insanity, since it possesses the power of producing it?

form a correct idea of the sense in which he intended it should be used. Unless its meaning be specially limited, the term can be applied to every possible degree of likeness, from that of almost complete identity to that of the vaguest resemblance. It is remarkable enough, that homœopathic authors, in discussing this point, seem to have overlooked those parts of Hahnemann's works, where he has attempted to fix the meaning of the word. 'We have only to oppose to this disease another disease as like it as possible, or in other words, a medicinal irritation analogous to the existing irritation of the disease, by the employment of a medicine which possesses the power of exciting as nearly as possible all those symptoms, or at all events the greater number and severest or most peculiar of them, and in the same order, in order to cure this disease we wish to remove.'[1] 'The choice of the medicine is not inappropriate, if the chief and most severe symptoms of the disease are covered in a positive manner by the symptoms of the primary action of the medicine, while some of the more moderate and slighter morbid symptoms are so only in a negative manner.'[2] 'Now the only desirable property that we can expect a medicine to possess is this, that it should agree with the disease; in other words, that it should be capable of exciting, *per se*, the most of the symptoms observable in the disease; consequently, when employed antagonistically as a medicine, should also be able to destroy and extinguish the same symptoms in the diseased body.'[3]

From these extracts it appears that Hahnemann considered a medicine as homœopathic, or similar, when it could produce 'the most or the greater number, or the most peculiar or severe, symptoms observed in any given case of disease.' But in perusing the histories of cases of disease published by homœopathic practitioners, and those brought forward in the *Organon* by Hahnemann as homœopathic cures, as well as in observing homœopathic practice at the bedsides of the sick, we find that the limited meaning assigned by Hahnemann to the word 'like' is completely ignored by homœopathic practitioners in general, and even by Hahnemann himself. We find, in short, that it is used to express every possible degree of resemblance, from

[1] Lesser Writings, p. 516; translated by Dr. Dudgeon.

[2] *Op. cit.* p. 540.

[3] *Op. cit.* p. 535.

almost complete identity, as the effects of snow to frost-bite, to the vaguest, or as Hahnemann calls it, partial similarity, as hooping-cough to measles.

From the want of a definite rule that would enable the practitioner, when tracing the resemblance between a medicinal and a natural disease, to decide what is *simile* and what is not, the application of the principle *similia similibus* becomes an arbitrary affair of individual opinion,—one finding the similarity great, where another considers it far-fetched and unsatisfactory. If to the want of accuracy and precision, naturally produced by the constant use of a vague and undefined term, we add that caused by the difficulty of forming a clear idea of medicinal diseases from the injudicious arrangement of their symptoms by Hahnemann, (of which we shall afterwards speak), we need not be surprised to find that homœopathic practitioners complain much of the serious obstacles which they encounter in the practical application of their principle. In short, even if it were well-founded, its formula would require to be changed, and the meaning of its terms so limited, that they could be used by its supporters in the same sense,—a condition without which the precision necessary to science cannot be attained.

The truth of these remarks is confirmed by what several homœopathic practitioners of undoubted merit have said on the same subject. I shall give a few extracts from their writings. Dr. G. Schmid, while admitting that, in the choice of a remedy, homœopathists must be guided by similarity of symptoms, says that 'one of the greatest difficulties in practice is to determine what this similarity really is.'[1] Dr. Dudgeon, one of the most talented of British homœopathic practitioners, says: 'And the discouraging thought has often struck me, if our knowledge of the relation of our therapeutic agents to the varieties of ophthalmia, where the symptoms are mostly objective and easily recognisable, be so vague and unsatisfactory, how much more so must be our knowledge of their relation to other diseases, where the symptoms are mostly subjective and uncertain!'[2] Again: 'However convinced we may be of the truth of the homœopathic law, its practical application is by no means always easy.

[1] *Brit. Jour. of Homœopathy*, vol. vii. p. 560.

[2] *Op. cit.* vol. vii. p. 1.

The pathogeneses of the Materia Medica sometimes afford us but the vaguest direction for our selection of a drug. Sometimes many medicines will appear to offer a closer correspondence to the case before us than the one which ultimately proves to be the suitable one. Again, the disease may be of such a sort, that there cannot be anything like an analogue to it in our repertory of medical diseases; for our provings cannot be carried to the production of serious maladies. In such cases, a good deal of the vaunted mathematical certainty of homœopathy is but guess-work, and as such is very apt to be unsuccessful. The *usus in morbis* which Hahnemann denounced, but availed himself of largely, is what we must look to to enable us to prescribe with certainty in almost every case, but especially in such as I have alluded to.'[1]

Trinks writes thus: 'The cause of this is, that the greatest possible similarity between the effects of the medicine and those of the disease, is sufficient, in a great many cases, to guide us in the choice of a proper medicine, in others only partially, and in many not at all.'[2] And the caustic Griesselich says of *Aehnlichkeit*—similarity: 'It is in some respects like a large bag, into which everything possible may be thrust.'

Before proceeding further, I think it will be advisable to define, and give a more precise meaning to, several words which will be frequently used in the course of this essay, and to which, even at the present day, rather vague and undefined notions are still attached. In medicine, as well as in other branches of knowledge, the use of such words has frequently impeded the satisfactory progress of science; indeed, I believe there is no circumstance that contributes so much to foster error, and that leads so often to tedious and useless discussions, as the use of ill-defined terms. The first word to be noticed, of which the meaning requires, if possible, to be defined with more clearness and precision, is LIFE.

At a time when researches in natural history were comparatively limited, and when physiological investigations were scarcely attempted, the phenomena of living bodies seemed so peculiar and wonderful, when compared with those of inanimate

[1] *Op. cit.* vol. xiii. p. 134.
[2] *Hom. Vierteljahrsschrift*, vol. ii. p. 303.

matter, that medical men fancied they could not be satisfactorily explained, unless by supposing the existence of some immaterial guiding principle in the body. Van Helmont developed that idea, and called the immaterial being which, as he supposed, pervaded the whole body, and regulated all its functions, the Archeus. Stahl, who was born in the year 1660, twenty-six years after the death of v. Helmont, modified considerably his opinions respecting the Archeus, and gave a more precise and scientific explanation of the phenomena of life. He called the spiritual, intelligent being which ruled the body in health and disease, the Anima.

During the latter half of the last century the knowledge of natural history and botany greatly increased; chemical analysis began to assume a more scientific form; and physiological investigation made considerable progress, thanks to the impulse given to it by Haller, who, rejecting the hypothetical opinions of Stahl, attempted to base all the phenomena of life on Irritability and Sensibility. These circumstances combined to make medical men more familiar with the phenomena of living bodies, to simplify their notions of them, and to facilitate the generalization of the rapidly accumulating facts of animal and vegetable life. They were thus led to modify essentially their ideas of it, without entirely abandoning the older notion of an immaterial governing principle. They retained the principle, deprived of its intelligence, and restricted in its power; some calling it the vital power, some the sentient principle, and others, using an almost synonymous term, the *vis medicatrix naturæ.*

During the present century, the opinions of medical men with regard to the vital force have become simpler, and more in harmony with the vastly increased amount of our knowledge of animal and vegetable life. Those who still believe in the existence of a vital principle have gradually substituted for the spiritual principle of former physiologists, the idea of a subtle material principle, without, however, being able to give us more precise and definite notions on the subject than their predecessors. Some of them have vainly attempted to show an analogy, if not identity, between the vital principle and electricity. But the great change which has taken place in the views of most physiologists on this point, consists in regarding the phenomena of life as simply the result of organization,—thus abandoning completely the belief in the existence of a special principle. This opinion has been gradually adopted by the most eminent of them.

It is only by means of our senses that we can acquire any knowledge of the external world. What does not reveal its existence to us by affecting one or more of our senses either directly or indirectly, is virtually to us as if it did not exist. I need not say that the existence of a vital principle is not revealed to us by any of our senses; and to infer its existence from the supposed peculiarity of the phenomena of living bodies, is simply to make an unwarranted assumption. *A priori*, no phenomenon in nature is more wonderful than another; and if any appears to us to be so, it is simply because we are not so familiar with it; or, in other words, the relation between cause and effect is not more mysterious in one case than in another: so that there is no reason why we should suppose the existence of some particular principle necessary to account for the origin of one class of phenomena, and not for that of another class.

If we could fancy our relative position to animated and inanimate nature reversed—that we were as familiar with the phenomena of the organic world as we now are with those of the inorganic, and as ignorant of the latter as we now are of the former,—it is clear that we should then find the wonderful or peculiar chiefly among the phenomena of the inorganic world; and it would be in order to explain their mysteries, that the existence of a special principle would be invoked. I may here advert to a circumstance that appears to have been generally overlooked—that the supposed existence of a vital principle, instead of simplifying the phenomena of life, and giving us a clearer notion of their production, would only increase the obscurity of the subject by increasing the number of points to be explained. Instead of saying that the poison of typhus fever acts directly on the nervous system through the blood, we should say it acts first on the vital principle, which being affected, acts in its turn on the nervous system; and thus, instead of one point of which we cannot give any explanation, we should have two equally obscure.

Nothing tends so much to simplify our notions of life, and to divest its phenomena of their mysteriousness, as the contemplation of what takes place in the lower scales of animal life, and especially in the different forms of vegetable existence. In the latter, transformation and assimilation of inorganic matter, circulation, secretion, and reproduction take place without our thinking it necessary to endow the productions of the vegetable world with a special principle, or to refer their phenomena to any other causes than the laws of organic matter.

Various definitions have been given of life by physiologists of the present century. Some of them are merely verbal; others, attempts to give a logical definition of the term, which cannot be successfully made, I think, in the present state of our physiological knowledge. One of the best which I have seen is that of Beclard, 'Organization in Action;' but the term organization (which itself requires definition) does not seem to be sufficiently extensive, and its condition is too limited. Among the protozoa we have life without organization, at least in the usual sense of the word. Even the amœba shows no decided differentiation of parts,—not the slightest trace of organs.

Eggs and seeds are admitted to be alive; so that in them we have life and organization without action. In such cases, it has been said, life is dormant; but the explanation is more poetic than scientific. In the abstract, however, it would be impossible to attach any meaning to the term life without action or motion, without its two essential functions, growth and reproduction. In the course of this essay I shall use the word life as an abstract term which comprehends all the phenomena peculiar to living beings.

The next word to which I shall turn attention is POWER, an abstract term, expressive of the relation of cause and effect. It is frequently employed without its signification being definitely fixed, so that it not rarely happens an author uses it in different senses, even in the same work, without apparently being aware of the circumstance. In using abstract terms, there is a great tendency to regard them as expressive of realities.[1]

[1] Of late years, the use of the abstract terms Force or Energy, instead of Power, appears to have become frequent in works of science. I doubt

Formerly the subject of causation was involved in much mystery, and it frequently gave rise to fruitless discussion. During the present century, simpler and more correct views have been taken of the subject; and at the present day, I believe the ablest men who have turned their attention to it, consider a cause to be simply the immediate and invariable antecedent of a change—the something without the presence of which the effect does not occur, and which being present *under the same circumstances*, the effect inevitably follows. In medicine, the old division of causes into proximate, efficient, occasional, and predisposing, was the result of erroneous views of causation; and it likewise tended, in its turn, to foster inaccurate notions on the subject. Correctly speaking, there can be only one cause of any given effect. An effect is the invariable and immediate sequence of any cause.

Health is a term which expresses that condition of the body in which all its functions, the structure and composition of its tissues and organs, and the quantity and composition of its fluids, are in a normal or natural state; disease, any deviation from that natural state, whether functional, or in the structure and composition of tissues, or in the quantity and quality of the fluids. It is impossible, in the present state of our knowledge of the animal economy, to draw a line of separation between healthy and diseased action, especially when the deviation from the normal state is slight; besides, it happens not rarely that states of the body, which are generally considered diseased or abnormal, seem to be the normal, or at all events the habitual, condition of some individuals.

It is evident that we cannot form a clear and correct notion of disease, until we become familiar with the phenomena of health, and with the laws which govern our bodies in the normal state; or, in other words, that we cannot form correct notions of the abnormal state, unless we have a clear and accurately defined natural standard with which we can compare it. Notwithstanding the great progress which physiology has made during the present century, so much uncertainty and obscurity

if any advantage be gained by using one of these terms instead of another. Our notion of the relation of cause and effect is not rendered clearer by simply changing the term which expresses it.

overhang almost every part of it, that we are evidently still far from the possession of a well-based system of pathology. The microscope, however, has already enabled us to reconstruct some parts of it almost entirely, and it promises to accomplish still more. Chemistry has likewise added largely to our knowledge of the subject; but I believe much of what it has contributed will yet be modified, or even abandoned.

Many able physiologists suppose that there is no such thing as functional disease. This opinion may be right; but until our knowledge of the nervous system be much more extensive than it is at present, it will be impossible to form a satisfactory opinion on this point. I think there cannot be any objection to the use of the phrase functional disease, if we limit its application to those cases in which no physical change can be detected in the part affected by the naked eye, or by means of the microscope. Those who do not admit the existence of functional disease, must take for granted, without being able to give any proof for their supposition, that none of the phenomena connected with the nervous system can be developed, unless some physical change takes place in it; that a voluntary muscle, for example, cannot contract in obedience to the will, or during the state of coma or sleep, unless the condition of the nerves which influence the contraction be physically changed.

CHAPTER II.

SECTION I.—TRIALS WITH CINCHONA AND QUININE ON HEALTHY INDIVIDUALS.

THE falsity of the fundamental principle of homœopathy can be proved only by showing that medicines do not produce in healthy individuals diseases similar to those which they can cure; or that they can produce in the healthy, symptoms similar to those of certain natural diseases, which, however, they are unable to cure. Its value as a therapeutic principle can only be determined by properly conducted trials on the sick, and will be considered in another part of this essay. Of the innumerable attacks that have been made on homœopathy, I know of one only in which the author has fairly grappled with the question, and shown by carefully and judiciously conducted trials, that medicines do not produce in the healthy, diseases similar to those they can cure. Dr. Jörg of Leipzig proved several medicines during the years 1822 and 1825; and his trials were admitted, even by homœopathic physicians, to be quite unexceptionable. The results which he obtained were quite opposed to those of Hahnemann.

A number of allopathic physicians of different countries have made individual trials of one or more medicines; but their provings have been conducted on too small a scale to have much value, taken by themselves. I should except, perhaps, those made by Andral and twelve medical students, of which he gave a verbal report at a meeting of the Academy of Medicine; but we have too few details about the manner in which they were performed, to be able to estimate them at their proper value.

The first medicine whose action on healthy individuals I shall examine, is cinchona. As far as regards the cure of intermittent fever, it is generally admitted that quinine is the efficient principle of the cinchona. I have therefore examined the action of the sulphate of quinine under the head of cinchona; but the provings of the two medicines will be given separately. I shall first give an account of the trials made by Dr. Jörg, in which every possible precaution was used to prevent errors, and of which he has published an account in a small pamphlet.[1] I shall describe them in his own words, without abridgment, as I shall afterwards have occasion to refer to them, when comparing the small number of symptoms noticed in allopathic provings of cinchona, compared with the large number recorded by homœopathic provers. Besides, some of my readers, perhaps, will be glad to have an opportunity of reading the details of the trials, as they will thus be enabled to judge for themselves of their value.

Seven persons took a part in the trials: they were all students of medicine, several of them were M.B.'s, and one of them had already taken part in some of Hahnemann's provings. In the first trial three students took a part: Ph. Enders, aged twenty-six, of choleric temperament, tall, constitution robust, but nervous system irritable; Hen. Hacker, twenty years of age, constitution robust, short but stout, temperament sanguine; F. Trautmann, aged twenty-three, habit of body slender, constitution healthy, temperament sanguine.

They began on Nov. 21st (1821), at 9 o'clock A.M., by taking in an ounce of water 18 drops of tinct. cinchon., prepared with 1 pt. of good cinchon. bark and 6 pts. of 80° sp. vini. The taste of the medicine was very disagreeable to Enders, who had taken a great quantity of bark about a year and a half previously, on account of intermittent fever. However, soon after the disagreeable taste disappeared, an agreeable feeling of warmth extended itself from the pit of the stomach to the navel, with tasteless eructations; no other effects were remarked. Hacker, after taking the dose, felt sick, with a disposition to vomit; had eructations, with discharge of flatus. Trautmann, immediately after taking the medicine, felt for about half an hour a gentle constriction in the throat, along with the peculiar taste of the medicine; an hour and a half afterwards, constant, bitter eructations till dinner-time, with an agreeable feeling of warmth

[1] Kritische Heft f. Ärzte, etc., 2 Heft, p. 148.

at the region of the stomach ; had, besides, frequent borborygmi till 6 o'clock P.M. The urine passed was of a darker colour than usual ; no stool during this and the following day.

On the 22d, 36 drops of the tincture were taken with two ounces of water, and the following effects were remarked:— Enders, a quarter of an hour afterwards, had four eructations of the taste of cinchona. On the 23d, contrary to his usual habit, his bowels were twice opened. Hacker felt sick after taking the dose, with eructations and frequent discharge of flatus; otherwise felt well. Trautmann remarked same effects as from former dose, with the exception of the constriction of the pharynx; the abdomen was a little distended for a short time, until eructations occurred.

On the 24th, at 9 o'clock A.M., each of the gentlemen took 70 drops of the tincture in two ounces of water. Enders felt comfortable after it; experienced the agreeable sensation of warmth in the region of the stomach formerly described; his appetite was very good; bowels were three times opened, last stool rather pultaceous. Hacker had less appetite than usual; frequent eructations and discharge of fetid flatus; complained of sickness; remarked that the veins of the skin were more prominent than usual. Trautmann, an hour after taking the drops, had sickness, constant eructations, bitter taste, mouth clammy, borborygmi, dark-coloured urine.

On the 25th, at 9 o'clock A.M., Enders and Hacker took 120 drops of the tincture in a tumblerful of water. The former, after he had got over the disgust caused by the medicine, and when its disagreeable taste had passed, felt himself well. At 1 o'clock P.M., however, soon after dinner, had considerable palpitation of the heart, increased by motion, lessened by quiet; it lasted till 7 o'clock P.M. Feeling anxious, he went to bed at a quarter past 8 o'clock P.M., and soon fell asleep, but frequently awoke. About 3 o'clock A.M. was awoke by considerable palpitation of the heart, which he tried in vain to relieve by frequent change of position; it was more severe when lying on the left side. His pulse was small, and more frequent than usual. The palpitation ceased in about an hour, and at 5 o'clock A.M. he fell asleep. About 9 o'clock A.M. of the same day, 26th, had a return of the palpitation, but in a less severe degree: it was increased by motion. In the evening he felt himself well again; no stool that day. Had a stool on the 27th, and on that day, as well as the following, the state of his health was good.

Hacker after taking the dose felt rather sick; had eructations, with discharge of fetid flatus; more liquid stools than usual; painful feeling in hæmorrhoidal swellings; prominence of veins of skin; had considerable pollutions four successive nights; otherwise felt well. Trautmann took 140 drops on the 3d of December, at 9 o'clock A.M., as usual; felt no appetite all day, but otherwise remarked nothing different from the effects of smaller doses.

The next experimenters were—Fr. Meurer, aged twenty-seven, of cholero-sanguine temperament, body short and stout, constitution healthy; and Con. Steinbach, aged twenty-three, constitution sound. Each of the experimenters took, at 9 o'clock A.M. on the 9th of December, two drachms of well-powdered cinchon. bark (*Cortex regius flavus*) in two ounces of water. Meurer, soon after swallowing the dose, had tasteless eructations, which continued till evening. About 11 o'clock A.M. had severe pain, lasting eight minutes, in the region of the stomach, and after two o'clock frequent discharges of flatus. Steinbach said that after taking the medicine he had several eructations, which were without taste or smell; took his dinner with appetite.

At 5 o'clock P.M. the same individuals took again two drachms of cinchona powder in two ounces of water. Immediately after Meurer took the dose eructations commenced, and continued more or less till he fell asleep. His sleep was uncomfortable, and he was disturbed four times during the night by erections. On the 10th no more eructation, but more troubled than usual with flatulence. During the night from 10th to 11th December twice awoke by erections. On the 11th December no effects from the bark. His appetite and digestion on the 9th, 10th, and 11th were not at all impaired—rather improved; in the secretions there was no notable change; not the slightest trace of a paroxysm of fever. Steinbach reported that soon after taking the medicine eructations began; had a stool in the evening more consistent than usual; slept well the following night. On the 10th and 11th he felt well, but was constipated till the 12th, when the stools became normal.

On the 10th December, a trial similar to the last was made by C. Klemm, aged thirty-four, of middle height, healthy constitution, sanguine temperament; and by E. Güntz, aged twenty-one, of short stature, robust constitution, sanguine temperament. Each of the provers took two drachms of cinchona bark at 9 o'clock A.M., and again at 5 o'clock P.M., in two ounces of water. Klemm remarked nothing during the day but constipation. The following day he felt well in every respect. Güntz from before dinner-time till 10 o'clock P.M. felt sickish, and had a sensation of weight at the stomach, but ate his dinner with good appetite. After the second dose, he experienced a feeling of pressure at the pit of the stomach; his appetite, however, was good, and his pulse was quiet; the following night he slept well. On the morning of the eleventh had two motions, but from that day till the 16th was constipated. On the 17th his bowels were in their usual order.

On the 18th December, Meurer, Steinbach, Klemm, and Güntz made the serious trial of swallowing, each, six ounces of tincture, prepared with an ounce of good cinchona bark, in order to see if intermittent fever would be produced by it, as Hahnemann had affirmed in the 105th paragraph of the *Organon*[1] that it infallibly would be. Each of the four took, at 9 o'clock A.M. (18th December), an ounce of the tincture in six ounces of water. Meurer, soon after drinking the medicine, had one or two eructations, and was thrown into an excited state in consequence of the alcohol. Steinbach felt slightly intoxicated after the dose — excited and merry till 10.15 o'clock A.M.; Klemm remarked no change in his state; Güntz said he had a sensation of warmth in the region of the stomach, and of constriction in the muscles of the pharynx; he likewise felt a little intoxicated; his pulse was 80.

At 11.30 o'clock A.M. each of the provers drank another ounce of the tincture as before, and all felt nearly the same effects as after the first dose. They all dined heartily. At 5 o'clock P.M. each of the provers took two ounces of the tincture with six of water. Meurer, Steinbach, and Güntz were again slightly intoxicated after the dose; Klemm not. All took their supper with pleasure. Güntz felt very sleepy about 8.30 o'clock P.M.

[1] *Organon*, 4th ed.

At 9.15 o'clock P.M. they all took again from me a dose of two ounces of tincture with six of water. They all felt well, but one could see they did not take the dose with pleasure. Meurer, after taking the last dose, had several eructations, but felt quite well. He slept very well the following night; awoke twice with a feeling of thirst, and, what was unusual for him, he had to pass water twice. Had no erections. On the 19th, felt his head somewhat affected, and during digestion slight colic; had two pultaceous stools. The following night he slept quietly, and on the 20th did not remark the slightest effect from the medicine, of which he had partaken so copiously; his appetite was good throughout the day; of fever there was no trace.

Steinbach likewise felt tipsy from the last dose, but otherwise felt well. As he was more lively than sleepy, he threw himself on the sofa, and smoked a pipe. Quite unexpectedly, at 11.15 o'clock P.M. slight vomiting came on. What he vomited was chiefly water of a very sour taste, and smelling of alcohol and cinchona. After that, his head was a little affected; slept several hours, but frequently awoke; in short, passed the night as if he had been drinking rather freely the preceding day. On the 19th felt his head still a little heavy, but without headache; other functions normal. On the 20th and 21st felt quite well. The action of his bowels had not been deranged during the trials.

Klemm informed us that, from the six ounces of this strong tincture, he did not remark the slightest change in the state of his health, which may be easily accounted for by the circumstance of his having, as military surgeon, made several campaigns, and been more accustomed to the use of spirituous liquors than the other persons who took a part in the provings. Güntz told us that, after taking the dose, he experienced a pleasant mixture of longing for quiet, and of gaiety. His pulse was 70; slept well during the night, and awoke at 6 o'clock A.M., with a feeling of slight exhaustion and headache, as after a sleepless night. By mid-day all that disappeared, and his appetite, bowels, and pulse were in a natural state; and neither during the rest of that day, nor during the following one, did he notice any change in his general state,—especially in the state of the pulse, or in the temperature of the body.

The next two trials which I shall notice were made by two individuals, and had direct reference to the paragraph of the *Organon* to which Dr. Jörg alluded in his last experiments.[1] Considered separately, they have but little value; but when added to those already made by Jörg, the results of which they confirm, their value becomes much greater.

At page 33 of the vol. of the *Bibliothek f. prakt. Heilkunde* for the year 1825, Dr. Naumann, in a review of one of Hahnemann's works, says that he made a trial on himself with the strong tincture of cinchona (1 oz. cinchona to 6 oz. of alcohol), as directed by Hahnemann, paying at the time great attention to his diet, and avoiding whatever was likely to have an influence on the action of the medicine, without remarking the slightest trace of intermittent fever.

The celebrated Kruger Hansen says, in his work *Allopathie und Homœopathie*, p. 289: 'That the swallowing of such a large dose of Hahnemann's tincture does not produce intermittent fever, I know by my own experience.' To which his friend Dr. Gross, the well-known homœopathic physician, replies, that 'every remedy requires a certain organic sensibility, in order that its specific action may manifest itself.' This remark might apply to exceptional cases, but not to those in which the phenomenon in question was the rule, and not the exception.

Before I became acquainted with Dr. Jörg's experiments, I had already tried the effects of cinchona and quinine on myself and five other persons, on a plan similar to, but more detailed than his. He never employed homœopathic doses in his trials; nor is there any reason to regret that he did not do so. Hahnemann and several other homœopathic practitioners have frequently employed them in their trials with medicines,—a plan decidedly and justly condemned by the majority of their brethren. I commenced the provings of some medicines with doses of the 1st, 3d, and 6th dilution or trituration, in order to see whether in such doses they could produce any effect. The medicine was administered once a day (beginning with the highest dilution, 6) for three successive days, doubling the dose on the second, and tripling it on the third day. The dose on

[1] That paragraph has not been printed in the last edition of the *Organon*.

the first day varied from 1 to 20 drops in a little distilled water. I may mention here, that the results of the trials [1] with infinitesimal doses were *always negative*.

The massive doses of cinchona were given in the form of a decoction, of which the strength was increased every three days. The decoction was prepared with ℥i. of bark, increased every three days to ℥iss. and ℥ii. successively, and 1 lb. of water; the last proportion used was ℥iiss. to ℥ix. of water. The dose of each decoction was ℥i. the first day, ℥iss. the second, and ℥ii. the third, taken morning or evening, according to agreement. As my object is not to describe the general action of the medicine, I may briefly remark, that nothing resembling a paroxysm of fever was produced in any of the individuals who took a part in the trials.

At a discussion which took place in the Academy of Medicine of Paris in the year 1835, in reference to a petition of homœopathic practitioners, which had been submitted to the consideration of the Academy by the Minister of the Interior, M. Bally stated that in the year 1801, Professor Dumas of Montpellier, and several other physicians, took various medicines, particularly bark, in a great variety of doses, during a period of four months, in order to try to produce fever artificially, but without success. During the same discussion, Andral said that he and twelve medical students had taken bark in homœopathic and in large doses, but intermittent fever was never produced.[2] The experiments spoken of at the Academy of Medicine, like Hahnemann's own provings, lose much of their value from the absence of requisite details respecting the doses of the medicines employed, the length of time they were used, and the number of persons who proved them.

A homœopathic physician,[3] Dr. Waltl, took daily cinchona bark during eight successive days. At first he took ʒii. of cort. chinæ fusc., and at last ℥ss. of it daily. During three days it produced no perceptible effect, but from the fourth his appetite became unusually great, and constipation occurred. Otherwise

[1] I tried cinchona, quinine, creta, sulphur, digitalis.

[2] In the vol. of the *Archives Generales de la Medicine* for 1835.

[3] *Allgem. Homœop. Zeitung*, vol. xx. p. 367.

he remarked nothing unusual during the trial—no feverish symptoms.

The last authority that I shall cite against the fever-producing property of cinchona is Professor Gandini of Genoa, who states, as the result of trials made on himself and several other persons, that there is no danger in using cinchona daily for a long time. Amongst the effects produced by its use, he makes no mention of intermittent fever.

I shall now proceed to give an account of some of the trials made on healthy individuals with the sulphate of quinine, almost the only medicine that has been repeatedly proved by allopathic practitioners. I shall first notice those made by Dr. Andral and twelve medical students, to which I have already adverted. He reports that they continued their trial of the medicine for a long time, in doses varying from 6 to 25 grains; but intermittent fever was never caused by it.

Professor Giacomini relates, in his work on Materia Medica, that at different periods during the winters of 1826 and 1829 he took repeated doses of sulph. of quinine, the total quantity taken being about two ounces. The trials were made in bed, with great care, from 9 o'clock P.M. till 2 o'clock A.M. The dose of the medicine was varied; but generally not less than 6 grains, and sometimes as much as 4 scruples, were taken during the five hours. During the first series of trials, the experiments were made daily for forty-six days consecutively. As my object is not to give an account of the general action of quinine, I shall not say anything of the general effects produced by it in Giacomini's trials: it will be sufficient to say that nothing like intermittent fever was produced by it. Similar experiments were repeated by Reviglio, and likewise by Bernaudi and Duval, with the same result, as far as regards the production of intermittent fever.

In a thesis published at Montpellier in the year 1848, Dr. Favier informs us that during ten days he took 18 grammes of sulph. of quinine, in quantities varying from 18 to 32 decigrammes; nothing like intermittent fever occurred.

Some time ago I tried the action of sulph. of quinine on five persons, of whom three were medical graduates; the other two were advanced medical students. The trial lasted about a month, a dose being taken nearly every day by each of the experimenters. Of the part of the trial where homœopathic doses were used, I shall say nothing. The larger doses were given once a day, in a little distilled water, and beginning with gr. i. were daily increased in the following manner:—gr. i., gr. iss., gr. ii., gr. iiss., gr. iii., gr. iiss., gr. iv., gr. v., gr. vi., gr. vii., gr. viii. Only two of the experimenters used the last four doses. As far as regards symptoms of intermittent fever, the result was completely negative.

I could cite many more trials made on healthy persons with sulph. of quinine; but I think I have brought forward sufficient data to warrant the conclusion that, in healthy individuals, the medicine referred to cannot produce any disease similar to intermittent fever.

SECTION II.—TRIALS WITH CINCHONA AND QUININE IN DISEASE—CASES IN WHICH CINCHONA OR QUININE WAS SUPPOSED TO HAVE CAUSED INTERMITTENT FEVER—RESULTS OF INVESTIGATION.

In addition to the account of the physiological trials, I shall now state some important facts with regard to its action in diseased states of the body, which confirm the results at which we have already arrived. The first case which I shall notice is that of Dr. Scott, who, having suffered long from very severe intermitting pain, probably connected with some organic disease in the abdomen, began by taking half grains of sulph. quinæ three times a day, adding 1 grain to the dose every other day, until he took 20 grs. for a dose, or ʒi. daily. Notwithstanding the rather alarming effects that it produced on the memory, Dr. Scott persevered in the use of the medicine until he took ℈i. four times a day. These large doses he could not use long, on account of the alarming effects which they produced—weakness of memory, falling down suddenly on the street, etc.; but symptoms of intermittent fever were not produced.[1]

Dr. Briquet says: 'In 300 cases of acute or chronic rheumatism, or typhoid fever, I have administered the sulph. quinæ in doses of 1 to 4 grammes in twenty-four hours. It was generally given gradually every hour in solution, and used in

[1] In *London Medical and Physical Journal* for March 1833.

every case for at least four days; in many for a much longer period.' Speaking of Hahnemann's statement, that cinchona can produce symptoms similar to those of intermittent fever, he says: 'It is an assertion so contrary to observation, that it would be useless to refute it. I have never observed the china fever, nor anything like it.'[1] Piorry, in the 6th vol. of *Traité de Medicine Practique*, p. 171, says: 'I gave it in every variety of dose to those ill of intermittent fever and other diseases, as neuralgia, hysteria; and although the patients were carefully examined by myself and by my pupils, I never saw this medicine, sulph. quinæ, produce shivering, heat, perspiration, and accelerated pulse.'

I am not aware that any cases which satisfactorily show the fever-producing power of cinchona or quinine have been recorded, with the exception perhaps of Hahnemann's own case, and another to which I shall immediately refer. In the homœopathic Materia Medica, it is true, the isolated symptoms of intermittent fever—shivering, heat, perspiration, etc.—are put down as effects of cinchona; but whether or not they ever co-existed, so as to form something like a paroxysm of intermittent fever, is a very different question. Hahnemann said that 'almost all medicines can produce a sort of intermittent fever;'[2] and certainly, in looking over the homœopathic Materia Medica, we find isolated symptoms of fever, as feeling of chilliness, shivering, etc., put down as pathogenetic effects of an immense number of different medicines.

I have not met with a homœopathic practitioner who could say that he had seen something like intermittent fever produced by the use of cinchona or quinine; and in a review of Dr. Jörg's trials with cinchona, in an early volume of the *Hygea*, by one of the editors of that journal, the only two cases that he was able to cite at that period (about the year 1835), in which the use of cinchona or quinine produced paroxysms like those of intermittent fever, are to be found in *Hufeland's Journal*. It is remarkable enough, that one so familiar with the homœopathic Materia Medica did not refer to Hahnemann's

[1] Briquet, *Du Quinquina*, p. 28.

[2] *Organon*, 3d ed. paragr. 257: 'fast jede Arzenei eine Art Wechselfieber errege.'

own case on that occasion. I think we may reasonably conclude that he did not find in it any satisfactory proof of the fever-producing power of cinchona.

The first case referred to by the reviewer was that of a girl seven years old, living in a country where intermittent fever was prevalent, and who had an attack of it two years previously. Since that time she had ten distinct attacks, sometimes of quotidian, sometimes of tertian, and even of quartan fever, at intervals varying from one to three months. The earlier attacks were removed by the use of some preparation of cinchona, and the more recent ones by the sulph. quinæ.

Two months ago she had a fresh attack, which took the tertian form. The parents thought it would go away of itself, and did not at first give the patient any medicine. As their expectations were disappointed, they sent for a physician, who found the disease to be febr. tertian. gastr., and he had no doubt it would yield, as formerly, to sulph. quinæ. Solvents and laxatives were given, to prepare for the use of the sulph. quinæ.

Soon after this, however, the paroxysms assumed a more threatening appearance, from the supervention of considerable coma at the time of their occurrence. Sulph. quinæ was therefore immediately prescribed in the following formula:—℞. sulph. chinini gr. i., pul. aromat. gr. ii., sacchar. gr. xv. M. D. S.; a powder to be taken every two hours in the intervals between the attacks. Nothing was remarked after taking the first and second powders; but soon after taking the third the patient began to complain of shivering, which lasted a quarter of an hour, followed by general heat which lasted half an hour, and afterwards by slight perspiration. The parents attributed this state to some irregularity in the occurrence of the paroxysm; and when it had terminated, the patient got up from bed. The same phenomena occurred after taking the fourth powder.

As the danger of the case had been pointed out to the parents, and as they were convinced of the efficacious action of the medicine in former attacks, they continued to give the doses. The usual paroxysm of the fever came on at the expected time, but much weaker, and without any comatose symptoms. The following day, the physician says, I witnessed the feverish attack after taking the dose; it lasted altogether about three-quarters of an hour. The powders were continued, and their effects gradually became less; the expected paroxysm did not come on, and since that time the patient has remained well.[1] It is not stated how many powders the patient used altogether, or how many she had used when the febrile attacks ceased.

I would remark, in the first place, that the individual whose case has been related was not in a state of health; and, again, we know that in persons who have recently suffered much from intermittent fever, a seemingly unimportant circumstance may bring on a paroxysm. I confess I am more disposed to ascribe the slight paroxysms which occurred in this case after taking the medicine to the overlooked influence of some such cause, than to the action of the quinine; at all events, it is remarkable enough, that the continued use of the supposed cause soon ceased to be followed by the supposed effect.

Dr. Osan, the co-editor of *Hufeland's Journal,* relates that a gentleman aged seventy, and his wife aged forty-five years, were attacked with nervous fever. They both got through the attack, but they could not regain their strength afterwards, although they had good nourishing food. The character of the husband was torpid; the wife had great irritability of the vascular system; she never had intermittent fever. A moderate dose (gr. ss. of sulph. quinæ) was ordered morning and evening. The lady got a decided attack of 'cold fever' after each dose. About an hour after taking the quinine, she was seized with shivering, which continued an hour; then came heat, followed by sweating, which continued several hours. The quinine was omitted, and no feverish attack came on.

Ten days afterwards it was supposed she would bear better a weak decoction of cinchona: ℥ss. of bark with ℥vi. of water was ordered. A single dose of one and a half tablespoonfuls brought on an attack quite similar to the former one. The decoction was left off, and she had no return of the paroxysm.[2] The husband used both preparations with benefit.

Dr. Osan does not state how many powders of quinine the

[1] *Hufeland's Journal der prakt. Heilk.*, vol. lxi. p. 140.

[2] *Hufeland's Journal d. prakt. Heilk.*, supplement. vol. (vol. lxi.), p. 96.

patient took. From the expression 'after each dose,' we should infer that she used more than one; but in speaking of the effects of the decoction, the writer says it brought on an attack similar to the former one, as if only one paroxysm had been produced. This is the only case I know of, with the exception perhaps of Hahnemann's own case, in which the use of cinchona and of quinine caused paroxysms of fever. When we consider the smallness of the doses employed, and how often much larger ones have been taken for a considerable time without any similar effects being produced, we must consider the present case as one illustrative of a rare effect of these medicines, or in other words, of an idiosyncrasy to their action.

In the year 1850, and again in the year 1851, M. Chevallier drew the attention of one of the sections of the Academy of Sciences of Paris to the effects supposed to be produced by cinchona or quinine on the individuals engaged in the manufacture of the sulph. quinæ. He informed the Academicians that Herr Zimmer, a chemist who had a large manufactory of quinine at Frankfort, had remarked that the workmen engaged in pulverizing cinchona bark were attacked with a peculiar fever—a sort of intermittent fever, which he called the cinchona fever (*china fieber*).

M. Chevallier made inquiries at the principal manufacturers of quinine in France respecting the occurrence of such a fever in their establishments, but the results of his inquiries were all negative. No one had observed such a fever. Dr. Guerard related to him a case, which occurred twelve years previously, of a workman employed at a manufactory of quinine, who was seized with intermittent fever: he was cured by the use of salisine after sulph. quinæ had failed.

The only other case of which M. Chevallier had heard was that of a workman, likewise employed at a manufactory of quinine, who got intermittent fever, but was cured of it in three days by small doses of sulph. quinæ. I need hardly remark, that there is no proof whatever that these two cases of intermittent fever were produced by the action of cinchona or quinine. Inquiries made on the subject of cinchona fever in Great Britain, at M. Chevallier's request, by Mr. Faraday and other friends, gave completely negative results. In Germany, Dr. Bückel was not more fortunate in his inquiries. If cinchona or quinine had the power to produce attacks similar to those of intermittent fever on the workmen engaged in quinine manufactories, such cases would have been observed in other manufactories besides that of Herr Zimmer.

I have lately found, in a work by Dr. Garms,[1] a letter from Herr. Mettegang, a long time overseer of H. Zimmer's quinine manufactory, in answer to a question of Dr. Garms, if in quinine manufactories, especially in that of H. Zimmer, the workpeople suffer frequently from intermittent fever:—'The diseases to which workmen engaged in the manufacture of quinine are subject are of two kinds:—First, the so-called cinchona fever, from which only those workmen engaged in the cinchona mill, and exposed to the cinchona dust, suffer. The illness shows itself by cold and heat, like intermittent fever. From observation, we know that it terminates with one violent paroxysm without the use of medicine. It is worthy of remark, that the workmen who have been once attacked can with impunity expose themselves to the action of cinchona dust. With few exceptions, this fever attacks all the workmen who inhale the cinchona dust. From the improved construction of our new mill, the workmen *no longer suffer* from the action of the dust.'

'The second affection is an eruption from which many of the workpeople suffer during the early period of their service, in connection with the subsequent steps of the process for making quinine. In a few, the eruption extends over the whole body; so that they are obliged to give up work, when they soon get well. When the eruption recurs on resuming their occupation, they are obliged to change it for some other trade. In general, however, the eruption disappears after some time; so that they resume their occupations and enjoy good health.

'If the illness is caused by the action of cinchona or quinine on the skin, or if it is owing to the action of the vapour of water, heat, acids, and spirits, I cannot say. The former is more probable, since the eruption may occur in persons engaged in any of the different steps of the manufacturing process—even in that of packing the quinine. The susceptibility to it varies in

[1] *Eröffnung eines neuen Weges*, etc., Leipzig 1853.

different individuals. The inhalation of cinchona dust never causes the eruption, only china fever.

'Our people do not suffer from diarrhœa, which has been remarked in some manufactories.'

It may be admitted that, in these exceptional cases, the inhalation of considerable quantities of cinchona dust gave rise to febrile symptoms, but it cannot be supposed that the dust was absorbed into the system; and consequently the cases did not realize the condition which Hahnemann considers the only sure one for proving drugs—their internal administration. We know already, that the production of febrile symptoms by the internal use of cinchona or of quinine is of very rare occurrence, although Hahnemann says, 'fast jede Arzenei eine Art Wechselfieber errege,'[1] or, as he has expressed himself at § 239 of the last edition of the *Organon*, 'As almost every medicine causes in its pure action a special peculiar fever, and even a kind of intermittent fever, with its alternating states, differing from all other fevers that are caused by other medicines.' So that even if it were admitted to be the rule, and not merely a rare exception, that the internal use of cinchona or of quinine caused febrile symptoms, almost any other medicine might be considered as suitable a homœopathic remedy in intermittent fever, as cinchona or quinine. The important point, however, to be remarked in the cases to which we have referred, is, that the characteristic sign of intermittent fever—the intermittence of the febrile symptoms—was not produced; and further, that, contrary to what is observed in cases of intermittent fever, the subject of the so-called 'china fever,' after having had one paroxysm of it, does not suffer from it afterwards, even when exposed to the action of the cause of the disease. It is to be regretted the account of the febrile symptoms, written by a person not of the medical profession, is rather vague and unsatisfactory.

A distinguished French physician—Dr. Bretonneau—says in one of his lectures: 'Daily observation shows that the use of cinchona in large doses causes, in a great number of individuals, a very decided state of fever (*mouvement fébrile*), of which the character and the period when it manifests itself vary in different individuals;' and MM. Trousseau and Pidoux, in their

[1] *Organon*, 3d ed. § 257.

work on the Materia Medica, seem to favour his opinion. But Dr. Bretonneau does not give any proofs in support of his statement, nor does he give any explanation of the term *mouvement febrile.* I am inclined to think that, in the present case, the difference of opinion between him and other physicians is to be sought for rather in the meaning which he attaches to the term, than in any difference of opinion between them with regard to the particular effects of quinine.

If Dr. Bretonneau regards as a state of fever the effects produced on the nervous system by large doses of quinine, such as heaviness of the head, headache, dimness of vision, noise in the ears, etc., and which some physicians term quinism or quininism, I think the great majority of medical men will consider his application of the word fever unwarranted. Besides, he appears to have overlooked a very important and frequent effect of quinine when used in large doses, as the experiments of Giacomini, Briquet, and others have shown,—the reduction of the normal frequency of the pulse from four to twelve beats in a minute, without being afterwards followed by unnatural acceleration of it, or by heat of skin. I think this phenomenon is sufficient of itself to invalidate Dr. Bretonneau's opinion.

At page 431 of the *Journal Hippocratique* for March 1840, Dr. Auber says, 'Piorry denies that sulph. quinæ ever causes an intermittent fever in healthy persons.' Although this may seem very remarkable, we can assure him that we have seen not a few instances of this effect, and we are happy to quote the authority of Goedorf, one of our most distinguished military physicians, in confirmation of our assertion: it results from experiments which that gentleman has made on himself, that the sulph. quinæ produces an intermittent fever in a healthy person. To this rather vague statement of Dr. Auber, I shall simply oppose a contradictory one by three well-known homœopathic physicians. In No. 146 *der Deutsch. allgemein. Zeitung,* Drs. Haubolt, V. Meyer, and C. Müller affirmed 'that it is impossible to produce intermittent fever by means of cinchona.'

I regret that I have not been able to see Wittmann's work, *Das Schwefelsaure Chinin, etc.,* in which, it is said, several experiments are related, which show that sulph. quinæ, administered to healthy persons in certain doses, produces a disease

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resembling ague. In a work by Dr. Garms,[1] however, which I have recently seen, I find an account of the cases cited by Asmus and by Wittmann in support of the fever-producing power of quinine, and likewise of the trials made by the latter physician on healthy individuals with the same medicine.

The case treated by Asmus was one of phthisis, in which, after the administration of 8 grs. of sulph. quinæ for a week, a most severe paroxysm of fever came on. The use of the quinine was continued; the paroxysms gradually became weaker, and soon disappeared entirely. This case is of no value as far as regards the present question, the power of quinine to produce in healthy individuals paroxysms of fever resembling those of intermittent fever, as the condition for proving the action of drugs—their administration to healthy individuals—was not fulfilled. The febrile paroxysms were more probably connected with hectic fever, which generally accompanies the advanced stages of phthisis, and of which the manifestations are often very irregular; besides, we cannot overlook the circumstance that although the administration of the supposed cause—quinine—was continued, the supposed effects soon ceased to appear.

Nearly the same remarks may be applied to the case treated by Wittmann. A person residing in Amsterdam, who had suffered long from intermittent fever, was taken ill with dropsical symptoms at Mayence, while on a journey to Schwabia. He was admitted into the hospital. The fever had disappeared. He was ordered 3 grs. of sulph. quinæ four times a day. He got again intermittent fever; but the remedy was continued, and the fever disappeared under its use. Enormous quantities of urine were passed, and the patient ultimately got quite well.

In order to test the fever-producing power of quinine, Wittmann gave it to three healthy individuals. The first—a healthy young man of twenty-four years of age, of a sanguine temperament—took 6 grs. of quinine in powder, fasting. No notable result was obtained. The second individual was a young man, aged eighteen, of nervous temperament. He took, fasting, 4 grs. of sulph. quinæ. Remarked only a slight chill, and a slight acceleration of the pulse.

[1] *Eröffnung eines neuen Weges zur sichern Indication der Arzeneimittel.* Leipzig, 1853, p. 404.

The third person was a robust peasant, aged twenty, of a lymphatic constitution, who took every hour two tablespoonfuls of a mixture containing 6 grs. of sulph. quinæ and 6 ounces of aq. menth. piperit. No effects remarked from its use. The following day he took three powders, each containing 6 grs. of quinine, in the space of nine hours; so that he took 24 grs. of sulph. quinæ in twenty-four hours. In the evening, after taking the third powder, he had shivering; pulse quick and frequent; dryness of mouth, with thirst; restless night; and on the following day there was a strong brick-red deposit in the urine. The digestive organs were not at all affected. Wittmann therefore concludes that sulph. quinæ, particularly in large doses, can produce effects similar to those of fever.

These trials, it must be admitted, are very incomplete and unsatisfactory. I think no one familiar with the proving of sulph. quinæ will admit that one dose of 4 grs. of that drug can produce even slight febrile symptoms. From one case (the third) we cannot safely draw a conclusion in favour of the fever-producing power of quinine. Had similar effects been noticed on repeating the trial several times, Wittmann might have been justified in concluding, that the use of quinine in large doses can occasionally produce febrile symptoms. As it was, the effects remarked might have been produced by some unobserved accidental cause; besides, the case is rather briefly related. It is not said how long the shivering lasted; nothing is said of heat of skin or of perspiration; and we know, from numerous observations, that retardation, instead of acceleration, of the pulse is a characteristic effect of large doses of quinine.

Long after the preceding part of this chapter had been written, I found in a German homœopathic periodical[1] a very elaborate and impartial article by Dr. Langheintz on the supposed power of cinchona or quinine to produce intermittent fever. He carefully analyzes almost all the published provings of cinchona and quinine, particularly those of Hahnemann, and the contributors to his provings. Dr. Langheintz says that Hahnemann first spoke of the power of cinchona to produce intermittent fever in a note to page 99, of the third volume, (2d ed.) of his work, the *Reine Arzeneimittellehre*. But although in the account of his first trial

[1] *Homœopathisch. Vierteljahrsschrift*, p. 419, vol. for 1865.

on himself with bark, of which I have given a translation, he does not distinctly say that the use of it produces intermittent fever, it will be admitted, I think, that the same ideas are expressed in other words: 'all the usual symptoms of intermittent fever appeared one after the other.' He likewise remarked, as he says, 'the usual, particularly characteristic symptoms of intermittent fever, dulness of the senses,' etc. In short, Hahnemann evidently considered the febrile symptoms produced by the use of bark to be as similar to a paroxysm of intermittent fever as one paroxysm of that disease is to another.

Dr. Langheintz justly remarks that the production of the isolated symptoms of a febrile paroxysm by the use of cinchona proves but little. 'We must know in what combinations they presented themselves, and how they were developed, before we can pretend to say whether or not the use of cinchona produced effects similar to a paroxysm of intermittent fever.' His conclusion is, that neither Hahnemann's trials, nor those of other experimenters, prove that cinchona, when used internally by healthy individuals, can produce intermittent fever; on the contrary, he says 'it must be admitted that the most of the provings which we have examined decidedly prove the contrary.' He might have added, nor can it cause, except in very rare cases, anything like a paroxysm of fever.

From a careful, and I trust impartial, examination of the facts brought forward to prove, as well as of those brought forward to disprove, the correctness of Hahnemann's opinion respecting the fever-producing power of cinchona, we must draw the conclusion that it is unfounded. I think I have amply proved that neither cinchona nor quinine, when given to healthy individuals, either in large or in small doses, can produce intermittent fever (what the majority of homœopathic practitioners of the present day probably admit), nor can they produce a state similar to a paroxysm of fever, except perhaps in some very rare cases.

Homœopathists often refer with pleasure to the circumstance that suggested the law of gravitation to Newton. They find a great analogy between the suggestion of the theory of gravitation to Newton by the falling of an apple, and the suggestion to Hahnemann of the therapeutic principle, *similia similibus*, by the production of symptoms resembling those of intermittent fever from the use of cinchona. In one essential point, however, the analogy is false: in 100 cases, when the apple is detached from the branch, and its subsequent movement unobstructed, it will invariably fall to the ground; but in 99 cases out of 100, in which cinchona has been administered to the healthy, neither intermittent fever, nor even a paroxysm of fever, will be produced. Had the apple not fallen to the ground in 99 cases out of 100, when it was detached from the branch, we should not have heard anything of the law of gravitation at the present day. Even if several well observed cases could be produced in which the use of cinchona or quinine caused symptoms resembling a paroxysm of fever, homœopathy would gain but little by their testimony,—they would only be exceptions to a rule. But although the homœopathic law has not been found to hold true, as far as the action of cinchona or quinine is concerned, it by no means follows that it does not hold true with regard to the action of other medicines. We shall therefore examine the action of some other drugs.

CHAPTER III.

TRIALS WITH SULPHUR ON HEALTHY INDIVIDUALS.—CONCLUSION WITH REGARD TO ITS ACTION.

AS sulphur is a remedy very frequently employed in homœopathic practice, and one whose action has been most carefully investigated by homœopathic practitioners, I shall select it as the subject of our present examination. I shall begin by giving an account of Professor Jörg's trials with it. Four new experimenters, all medical students, besides five of those who had been engaged in proving cinchona, took a part in the trial: E. Kneschke, aged twenty-three, short, stout, of sanguine temperament; R. Kind, aged twenty-one, short, stout, of sanguine temperament; C. Hartlanb, aged twenty-four, of middle hight, habit of body rather slender, temperament melancholico-sanguine; S. Gutmann, aged twenty-eight, tall, temperament cholerico-sanguine. The flores sulphuris were used, and with each dose of the drug a similar quantity of sugar was taken: the doses were always taken at 9 o'clock A.M.

At 9 o'clock A.M. on the 8th of January, Enders took 5 grains of fl. sulphur., on the 9th 10 grs., on the 11th 15 grs., on the 13th ℈i., on the 15th ʒss., on the 17th and 19th the same quantity; on the 21st, 26th, 30th, and 1st of February, ʒi. each time. Each dose speedily acted on his lungs like sulphur vapour; for about a quarter of an hour he had oppression of the chest, more marked after the larger doses. After the third dose the action of the skin was increased, and he continued to perspire more than usual, while the trial lasted; his perspiration and linen had an odour of sulphur; the stools were more frequent than usual, soft, pultaceous; their odour resembled that of sulphuretted hydrogen. The urine passed more frequently, but less copiously, than usual, and had likewise an odour of sulphur. He felt no itching of the skin, nor was any trace of itch or other eruption remarked.

Hacker, at 9 o'clock A.M. on the 9th of January, took 5 grs. of fl. sulphur., on the 10th 10 grs., on the 11th 15 grs., on the 13th ℈i., on the 15th ʒss., on the 18th ʒi., on the 21st and 29th the same each time, and on the 5th of February ʒii. The following were the effects observed: discharge of offensively smelling flatus: the action of the bowels was not increased; on the contrary, from January 29th he was constipated three days. On the 21st, felt for a short time slight itching in the thighs. On the 5th of February, contrary to his usual habit, perspired towards morning; thinks that, since the commencement of the trial, he has passed more urine than usual. In other respects, felt quite well; no traces of itch or any other eruption remarked on examination of his body.

Meurer took, at 9 o'clock A.M. on the 9th of January, 5 grs. of fl. sulphur., on the 10th 10 grs., on the 11th 15 grs., on the 13th ℈i., on the 15th ʒss.; on the 17th, 19th, 23d, 26th, and 29th, ʒss. each time; on the 1st of February he took ʒi. of the medicine, after which he had frequent discharges of flatus, of the odour of sulphuret. hydrogen. On the 11th of January, contrary to custom, had a stool in the evening, but on the following morning he had not his usual evacuation. From the 13th to 18th, had generally two evacuations daily, of a sulphury odour, and frequent discharges of flatus, without any other noticeable symptoms. On the 18th, remarked slight pain in chest, with a feeling of oppression, and occasionally a dull stitch, which was more acute on the 19th. On the 21st had two stools, with pain in rectum; but the pain in the chest remained, although the stitch was felt only thrice during the day: discharge of flatus as before. On the 23d, a stool in the evening; almost no pain of chest, or flatulency.

On the 23d, after a long interval, he took again ʒss. of the medicine. Had again two evacuations daily, with pain in rectum and chest, along with violent stitch and considerable oppression, until the 25th, when he got his feet severely chilled during rough weather. On the 26th, 27th, and 28th, had daily two pultaceous stools, without any sensation of burning at the anus; the oppression of the chest continued, but the dull stitch recurred only a few times on the 27th. On the 29th, 30th, and 31st, he remarked a continuation of the same symptoms,—pain in the chest, with oppression and occasional stitch; the stools and flatus as before—more frequent than natural, and of a sulphury odour.

The same state of things continued on the 1st and 2d of February; on the 3d the affection of the chest disappeared, and on the 4th he felt again quite well. In the perspiration and urine Meurer did not remark any change, either as regards quantity or composition. He supposed, however, that the sulphur acted on his skin, as he always felt less pain of chest when he remained more in his room and exposed himself less to the inclemency of the weather. His appetite and digestion were not affected during the whole period of the proving. Itching of the skin had not been remarked by him; nor had any eruption been noticed, although we often looked for one.

Klimm took, at 9 o'clock A.M. on the 10th of January, 10 grs. of sulphur flowers, on the 11th 15 grs., on the 13th ℈i., on the 15th, 17th, and 19th, ʒss.; on the 21st, 26th, and 28th of January, and on the 1st and 4th of February, ʒi. each time. On the 10th and 11th of January he felt, about an hour after taking the dose, a burning pain in the chest, with short breathing; these symptoms, however, lasted only two hours. On the 13th these effects were not remarked, nor did they reappear after taking the larger doses; but on that day he had two evacuations, with increased discharge of sulphurous flatus, and the same effects occurred every day, as long as he used the sulphur. We did not remark any eruption on his skin during the whole time of the proving.

Güntz took, at 9 o'clock A.M. on the 15th of January, 10 grs. of fl. sulphur., and on the 17th ℈i., without perceiving the slightest effect from the drug. On the 19th he took ʒss., after which he had three liquid stools, but found himself otherwise well. On the 23d took again ʒss. fl. sulphur., from which he had four liquid stools, and on the 24th three similar ones. On the 2d of February he increased the dose to ʒi., and the same day he had two pappy stools, but otherwise there was not the slightest change in his condition. During night, from 1st to 2d of February, slept unquietly; and on the 2d felt flying stitches in breast.

On the 8th he repeated the same dose (ʒi.), after which he had two consistent motions. Towards evening of same day, felt chest a little oppressed. He passed the night, from the 8th to the 9th, very restlessly; could not sleep much; chest oppressed, and he had flying stitches in it, which lasted till the evening of the 9th. On the 10th, stitchy pain was remarked in chest, which continued until the 15th, when he felt himself quite well again. No eruption on the skin, and no itching of it.

Kneschke took, at 9 o'clock A.M. on the 9th January, 5 grs. of fl. sulphur., and on the 10th 10 grs., without remarking any effect from them. On the 11th he took 15 grs., and remarked afterwards considerable discharge of flatus of a sulphurous odour, and his bowels were moved once more than usual; stool of same odour as flatus. On the 12th, in the same state, with this exception, that there was no discharge of flatus. On the 13th, after taking ℈i. of the drug, the discharge of fetid gas again returned; had one stool more than usual, thinner, of a yellowish colour, smelling like rotten eggs: the same effects were noticed on the 14th.

On the 15th he took ʒss. of the drug, which made the evacuations more frequent, pappy, and of a most offensive odour, like that of the flatus discharged. On the 17th repeated the ʒss. dose, and remarked similar effects from it. On the 18th his usually good appetite had increased so much, that his usual quantity of food was insufficient to satisfy it. The same day he had two evacuations, with considerable discharge of flatus. On the 19th he repeated the ʒss. dose, with the same result; on the two following days had three evacuations daily; his appetite was very keen, and his urine, of a darkish colour with sulphurous odour, was increased in quantity; he occasionally felt a sulphurous taste in the mouth, and during two days, the 20th and 21st, a slight feeling of tightness in the chest, especially in the morning. On the 22d all these symptoms had disappeared; so on the 23d he took again ʒss. of fl. sulphur., and during two days had two yellow, pappy, offensive evacuations daily; his appetite was excellent; his urine less yellow, was copious, and had the sulphurous odour which at that period his whole person emitted: the oppression of the chest did not return.

On the 26th took ʒss., with former effect on the bowels; but the urine was paler and less copious, and had less odour than formerly. The chest remained free; he was always hungry. On the 29th he took ʒi. of the drug, and was afterwards purged three times daily, till the 1st of February. All the evacuations had an odour of sulphur; felt slight oppression of the chest; appetite was excellent. On the 1st of February repeated the dose of ʒi., and remarked after it the same effects as last time, with the exception of the affection of the chest, which did not recur. On the 4th all the symptoms produced by the sulphur had disappeared. Of eruption, neither he, nor any of us who had examined his skin, remarked any appearance; and he assured us he had not felt any itching of the skin.

Kind and Hartlanb, who used the same doses at the same periods, briefly reported that the use of the sulphur made their stools softer, but not more frequent, preceded by a feeling of itching in the anus, like what may be observed in diarrhœa; they had frequent discharge of flatus of an odour more like that of carburetted than of sulphuretted hydrogen. Hartlanb felt almost constantly, during the proving, a stitch in the left side; Kind had nothing like it.

Gutmann began, like the others, with 5 grs., and gradually increased the quantity to ʒi. He gave in a long list of symptoms supposed to have been produced by the medicine. Essentially his observations agree with those of the other experimenters; especially in this respect, that no eruption was to be seen on frequent examination of his body during the proving.

I shall now give a brief account of some provings which I made with sulphur on myself and five other individuals. One half of the experimenters took a dose once a day in the morning; the other half took the drug in the evening. Each prover began by taking three doses of the 3d trituration of sulphur, containing 1, 2, and 3 grs. respectively, on three successive days; then the same quantity of the 1st trituration was taken for the same period, and afterwards, 10, 15, and 20 drops of the strong tincture of sulphur, on three successive days. We next used massive[1] doses of fl. sulphur., commencing with a dose of 10 grs., adding daily 10 grs. to each successive dose, until ʒi. of the drug was

[1] In contradistinction to infinitesimal.

taken at a time. The effects remarked from the use of the drug resembled very much those indicated in Jörg's experiments, with the exception of the oppression of the chest, which was not remarked even once in our provings. More or less marked tension of the abdomen, owing to the increased formation of gas in the stomach and bowels, was of frequent occurrence during the use of the massive doses; but no sensation was felt that could be called oppression of the chest. Although the sulphur exercised a decided action on the skin, we did not remark any itchlike eruption on any of the experimenters.

While using the massive doses, one of the gentlemen felt a slight itching for a couple of days about the hips and upper part of the thighs, and at the same time he remarked a few small isolated papulæ on the parts that itched; they disappeared in two or three days. In my own case, during the use of the massive doses, there was slight itching of the skin generally which made me scratch myself a little, especially on going to bed and on getting up in the morning; but there was no appearance of any eruption.

Another of the experimenters, aged twenty, of nervous temperament, short stature, and slender habit of body, subject to catarrhal affections, but not to eruptions of the skin, showed us, after finishing the doses of the 1st trituration, a few pale-coloured papulæ on the forefingers and adjacent parts of the back of both hands (chiefly the right one). No vesicles could be detected on them, and they did not itch. After having occasionally disappeared and reappeared, they ceased to show themselves about ten days after their first appearance. Were they occasioned by the action of the drug, or by the accidental application of some irritating substance to the parts during manipulations in the dispensatory, in which he sometimes took a part? Unfortunately we had no opportunity to make further trials of the sulphur on this individual. Having caught cold after finishing the dose of the 1st trituration, and his nervous system being deranged by fatiguing application to his studies, he was advised to discontinue the provings.

In his account of the pathogenesis of sulphur in his manual of homœopathic practice, Jahr says nothing of its power to produce an eruption resembling the itch.

No medicine, probably, has been so frequently administered by itself as sulphur; and when we consider how extensively it has been employed, both as a prophylactic and as a means to remove diseases, it is impossible to suppose, if its use even occasionally caused an eruption similar to that of itch, that a circumstance so important, an effect so visible, could have been overlooked both by patients and by their medical attendants.

It is generally admitted, at the present day, that the itch depends on the presence in the skin of an insect, the *acarus scabiei*, and that the disease cannot be communicated by inoculation with the fluid of the itch vesicle or pustule. We can easily understand, then, why the use of sulphur cannot produce the itch; and although it may occasionally cause more or less itching of the skin, and in rare cases a few isolated transient papulæ or pimples, no one could pretend to find any but the vaguest resemblance between the effects of sulphur and the itch. It appears, then, the homœopathic law, that drugs cure diseases by the power which they have to produce similar diseases in healthy persons, does not hold true in the case of sulphur any more than in that of cinchona.

I should add, the opinion of homœopathists, that itch can be cured by the internal use of sulphur, is very probably unfounded; in which case the supposed production of something like itch in healthy individuals by the internal use of sulphur would tell directly against the principle, *similia similibus*. If the two propositions—(1) that the internal use of sulphur cannot cure the itch; and (2) that its internal use by healthy individuals cannot cause anything like it—be true, no conclusion can be drawn from them, either for or against the homœopathic principle.

CHAPTER IV.

EFFECTS PRODUCED BY MERCURY ON WORK-PEOPLE IN VARIOUS INDUSTRIAL OCCUPATIONS—COMPARED WITH THOSE OF CONSTITUTIONAL SYPHILIS—CONCLUSIONS FROM PROVINGS OF DRUGS CONFIRMED BY OTHER FACTS.

THE next medicine whose action I shall examine with reference to the homœopathic law is mercury. I select it for examination, because it is one whose supposed action seems to give stronger support to the homœopathic principle, *similia similibus,* than that of any other drug with which I am acquainted. I shall afterwards have occasion to show that in some respects there is a great resemblance between the effects of mercury on the system and constitutional syphilis; but the points of resemblance are not so numerous as some of the current opinions respecting the action of mercury would lead us to suppose. The researches of Virchow, Waller, and particularly those of Overbeck and Kussmaul, have thrown much light on mercurialism and constitutional syphilis. They have most satisfactorily exposed the inaccuracy of some of the opinions generally held with regard to the action of mercury,—such as its supposed action on the bones; and of the more peculiar opinions of Keller, Hermann, and Lorinser, who disbelieve the existence of constitutional syphilis, and ascribe the so-called secondary and tertiary symptoms to the action of mercury. I do not believe that any satisfactory trials have been made to determine the action of mercury on healthy individuals, and for a very evident reason. The account given of its action by homœopathic as well as by allopathic practitioners seems to have been derived chiefly from observing its effects in disease—*usus in morbis,* from cases of mercurial poisoning, or from experiments on animals.

It so happens, however, that certain industrial occupations, in which healthy individuals are exposed to the action of mercury in various ways, afford us ample opportunities for studying its effects. The work-people employed in quicksilver mines, in certain processes of the manufacture of looking-glasses, etc., very frequently suffer severely, and for long periods, from exposure to its injurious action.

As it is not my object to enter into the discussion of the value of conflicting opinions respecting the properties of mercury, I shall merely endeavour to give a correct summary of its action, derived from the most recent and trustworthy investigations. It does not appear that there is any essential difference in the effects produced on the system by the different forms or preparations of mercury. It is worthy of remark, however, that when mercurialism is produced by exposure to the influence of mercury during some industrial operation, tremor of the muscles is much more marked, and stomatitis much less so, than when these effects are caused by the use of mercurial medicines. In order that the reader may be able to compare more easily the points in which constitutional syphilis and mercurialism resemble, as well as those in which they do not resemble each other, I shall arrange their more marked symptoms in a tabular form.

I. *Points in which Mercurialism and Syphilis agree, or resemble each other.*

1. Both cause an anæmic or chlorotic state of the blood, with disposition to hyperæmia and inflammation.
2. Each may cause catarrhal affections of the mucous membranes of the ears, of the larynx, and likewise of the eyes. Some cases of pure mercurial chronic stomatitis occur, in which small superficial ulcers appear on the mucous membrane of the mouth and tonsils, with copper-coloured hue of the surrounding membrane. Such cases cannot be distinguished from those of syphilitic origin by their appearance alone.
3. Both cause derangement of the menstrual function, and predispose to abortion.
4. Flying, rheumatic-like pains in various parts of the body, without any perceptible physical change in the muscles,

fibrous membranes, or ligaments, may be produced by both in the earlier stages of their action.

5. The memory is sometimes affected in both diseases.
6. Both poisons, mercury and syphilis, may remain latent in the system for a longer or shorter time, to recommence their action with more or less severity.

II. *Points in which they differ, or do not resemble each other.*

CONSTITUTIONAL SYPHILIS.	MERCURIALISM.
1. Is a contagious disease; may be communicated by inoculation with the blood and various secretions of a diseased person.[1]	Not contagious.
2. More or less general enlargement of the lymphatic glands.	Not remarked, except occasionally in those of the neck, in connection with stomatitis or angina.
3. Affections of the skin the most frequent of secondary symptoms: in 247 cases of secondary syphilis, Engelsted found skin diseases in 90·5 per cent.; distinguished by their colour, form, seat, and long duration; frequently leave traces after them.	In mercurialism they scarcely amount to 1 per cent. of the cases; not of long duration, generally disappearing after some days, without leaving any trace behind them; usually assume the form of boils, urticaria, erythematic patches, or more frequently of eczema;[2] in syphilis, these forms are unimportant.

[1] With the secretions from broad condylomes and rhagades, of acne and ecthyma pustules. Prager *Vierteljahrsschrift*, 1851, vols. i. and iii.

[2] Eczema is most frequently produced by the external use of mercury; and it occurs so seldom from its internal use, that many practitioners doubt the existence of such cases. Some of the most severe cases of eczema, in which the eruption spread itself over the whole body, have occurred in individuals who had not used mercury.

4. Condylomata of frequent occurrence.	Do not occur.
5. Ulcers of skin of frequent occurrence.	Seldom observed, except under the form of varicose ulcers of legs.
6. Gummy tumours pretty frequent in the cellular tissue and other parts.	Never observed.
7. Ordinary stomatitis with salivation not observed; ulceration of mucous membrane of frequent occurrence, but generally differing from that of mercurialism in appearance and seat.	Stomatitis with ulceration and salivation of frequent occurrence; if severe, diphtheritic exudation occurs on the mucous membrane, with suppuration and sloughing of the soft parts, periostitis, and subsequent necrosis of the jaw-bones.
8. Stomach and intestines seldom affected.	As far as regards constipation and diarrhœa, intestines frequently affected.
9. Giddiness seldom remarked in the early period of constitutional syphilis.	In mercurialism it shows itself early; is frequent and severe.
10. Seldom if ever noticed.	A strong feeling of distress and precordial anxiety, with rapidly increasing muscular tremor, and even loss of voice, are rarely absent at the beginning of mercurialism.
11. Syphilitic paralysis seldom accompanied by decided tremor, and still less frequently preceded by it; it assumes the	Mercurial palsy takes almost always the form of paresis tremens, generally gradually developed; but under the influ-

form of paraplegia, but more frequently of hemiplegia, and palsy of particular nerves; sometimes occurring suddenly, sometimes preceded by paresis of long duration.	ence of strong emotions, it may be rapidly increased, affecting sometimes one set of muscles, sometimes another; seldom occasions complete palsy of an extremity, and even in these cases it is preceded by tremor.
12. Palsy of the nerve oculomotorius and its branches, and that of the nervous abducens and facialis is not rare. Von Græfe says that the half of the cases of paralysis of the nerves of the muscles of the eye is owing to syphilis.	Even in the severest cases, palsy of these nerves never observed.
13. Epilepsy is one of the most frequent syphilitic affections of the nervous system.	No well-marked case of chronic epilepsy produced by mercury has yet been cited.[1]
14. Softening of the brain, with or without meningitis, is not rare; sclerosis and gummy tumours[2] may occur in the brain in advanced syphilis: these lesions are generally confined to its anterior lobes.	These lesions have not been remarked in mercurialism.
15. Choroiditis, keratitis, and particularly iritis, are frequent enough.	Not remarked in pure mercurialism.

[1] At page 312 of his work, *Mercur und Syphilis* (1861), Overbeck says: 'Das Dasein einer wirklichen Mercurialepilepsie ist damit freilich noch nicht bewiesen; darüber kann eben nur weitere literarische Nachforschung Gewissheit verliehen.' Kussmaul, at page 366 of his work, *Untersuchungen über den constitutionellen Mercurialismus* (1861), says: 'Ueber das vorkommen einer rein ausgebildeten, wirklichen d. h. chronischen Epilepsie scheint mir zür Zeit nich hinreichend constatirt, obwohl ich die Möglichkeit derselbe nicht abstreiten will.'

[2] Gummy tumours may likewise occur in its membranes, and in other organs and tissues.

16. Affections of the periosteum and bones are frequent in the more advanced periods of the disease.	The latest and most careful investigations have shown that, contrary to the opinion generally received, they do not occur except in the jaw-bones, where periostitis and subsequent necrosis of the bones have been often observed in connection with stomatitis and salivation.[1]
17. Ozæna and ulcers of the larynx not infrequent.	Have never been remarked.
18. Amyloid or fatty degeneration may occur in the liver, testicles, and other organs.	Never observed, unless phthisis be previously developed.
19. Does not set in suddenly.	Severe mercurial tremor may be suddenly developed, even in the course of a night.
20. During its progress it may present considerable variety in its form.	Mercurialism, even when it has lasted for years, presents little change.
21. The system, once affected by it, is more or less protected from future infection.	Mercurialism predisposes the patient to subsequent attacks of it.

[1] Waller, in an article published in the 63d vol. of *Canstatts Jahrsber.* (1859), entitled Beiträge zur Lösung, etc., says: 'Der Beweis für mercurielle Knochenleiden ist erst noch zu liefern.' Virchow, in the 15th vol. of *Archiv. f. path. Anat.* p. 223, etc., remarks: 'Erste weitere Beweise abzuwarten, bevor Man die mercuriellen Knochenaffectionen als eine wohlconstatirte Thatsache zuläst.' Overbeek, in his work *Mercur und Syphilis*, p. 188, says: 'es giebt keine mercurielle Knochenkrankheiten.' Kussmaul, at p. 335 of his work referred to in preceding page, says: 'Ausser der Periostitis mit nachfolgender Necrose der Kieferknochen im gefolge der Stomatitis Mercurialis sind zur Zeit keine mercuriellen Knochenleiden constatirt.' Swan, Bretonneau, and Overbeck made numerous experiments in order to try to produce mercurial action on the bones of animals without success.

On examining these tables, it will be evident that, although several of the symptoms of constitutional syphilis and mercurialism are very similar, the great majority of the symptoms of the former disease have no resemblance to those of the latter. As far as regards the most striking and characteristic features of each, such as the contagious nature of the disease, salivation, form and frequency of skin affections, paralysis of the muscles of the eye, and the affection of the periosteum and bones, they differ completely. Let us apply Hahnemann's test of similarity, which we noticed in a former part of our work,—'as nearly as possible all the symptoms of the disease, or at all events the greater number, and severest or most peculiar.' Do the effects produced by mercury cover the greater number, or severest, or most peculiar symptoms of syphilis? It will certainly be admitted that they do not.

It is worthy of remark here, that the action of lead, although it does not resemble that of syphilis on the mucous membrane of the mouth, resembles it more than that of mercury does in several other respects, such as the frequency of its well-marked epileptic cases, the occurrence of complete muscular palsy, and the absence of that anxiousness and timidity so characteristic of mercurialism. We cannot suppose, then, that the power of mercury to cure syphilis is owing to the partial resemblance between its effects and those of syphilis; since lead, whose action bears about as much resemblance to the symptoms of constitutional syphilis as that of mercury does, cannot cure that disease. In short, to use the words of Kussmaul, 'constitutional syphilis, as far as regards its course and symptoms, is a disease essentially different from constitutional mercurialism.'

We find, then, on examining the action of mercury, what we found on examining that of cinchona and sulphur, that the results of observation (and likewise of experiment, as regards the latter two medicines) are decidedly opposed to the homœopathic principle, *similia similibus curantur*. As we have arrived at the conclusion that drugs cannot produce on healthy individuals symptoms similar to those which they can cure, from an examination of the properties of these very important ones, it would be useless at present to extend our investigations; or to show that some drugs can produce symptoms similar to those of certain natural diseases, which, however, they cannot cure; that strychnine, for example, can produce symptoms similar to those of tetanus, a disease which it cannot cure.

Unless some important errors can be pointed out in these investigations, it must be admitted they amply prove that the principle *similia similibus curantur* is a false induction. The truth of this conclusion is confirmed by two important facts, which I shall now proceed to notice.

We cannot overlook the circumstance that Hahnemann himself admitted the insufficiency of the principle *similia similibus* as a guide in the treatment of *chronic* diseases, although, when he first announced his new plan of treatment, he considered it particularly applicable to them. I do not mean to enter upon an examination of his opinions with regard to the nature of chronic diseases: my object is only to show that he felt himself obliged to admit that the remedies most appropriately chosen, according to the homœopathic principle, were unable to cure seven-eighths of all chronic diseases. In the introduction to his work on *Chronic Diseases*, he says: 'The commencement of the treatment inspired confidence, its continuation produced effects less and less favourable, and its termination destroyed all hope.' 'But the truce was never of long duration: frequent relapses finished by rendering the drugs most homœopathically chosen, and in most appropriate doses, so much the less efficacious the more frequently they were repeated.' 'In general, after repeated efforts to conquer an affection which always reappeared with some new modifications, there remained, even when the patient had attended sufficiently to his regimen, and had fulfilled punctually all that was prescribed, complaints which the most approved medicines could neither remove nor even diminish, and which, by incessantly multiplying themselves, became more and more troublesome;' 'that *the impossibility to cure homœopathically certain affections* was but too evidently owing in most cases to a previous attack of itch.'

It has been said that homœopathic practitioners are still guided by the principle *similia similibus* in their choice of medicines for the treatment of chronic diseases. But Hahnemann has distinctly stated that this principle was insufficient to guide them in their selection of remedies; and it was in order to complete the deficiency that he framed a plan of treatment, now generally employed, based on the supposed nature of the cause of chronic diseases—the itch miasm. It is true that, in doing so, he acted in direct opposition to the opinion he formerly so strongly expressed about the folly of attempting to discover the causes of disease.

There is a numerous and important class of diseases, to which the homœopathic principle, that drugs cure diseases by virtue of their power to produce similar morbid states when administered to healthy individuals, cannot be applied—I mean organic diseases. No one could be foolish enough to push the proving of medicine so far as to produce organic disease, even were it possible to do so. A diseased state may be easily and certainly produced by most drugs; but as far as our knowledge of the action of medicine goes, we have no reason to believe that any organic disease, such as tubercle, or fibrous tumour, cancer, etc., can be produced by the direct action of any drug on a healthy individual; so that the homœopathic principle, even if its truth had been established, is evidently inapplicable to organic diseases.

Many cures, however, of supposed organic diseases are to be found in homœopathic records. Thus, in the report of the homœopathic hospital which formerly existed in Munich, we read of 19 cases of phthisis, of which 13 are reported cured. Even Dr. Fleischmann cannot admit the truth of this remarkable statement, and wonders how they managed their consumption cases at Munich, for at Vienna all his died.

I believe few homœopathic practitioners now suppose that the celebrated case of Field-Marshal Radetzky was one of genuine carcinoma of the lachrymal gland. Had it been so, we should certainly have heard of many more cures of carcinoma since the time when homœopathy claimed that famous one. But, in short, we have no reason to believe that the action of any drug can produce so-called organic disease, and without that condition no treatment can be called homœopathic. Dr. Elbe, a homœopathic practitioner, says: 'It is impossible, in a disease conjoined with material changes, to choose the remedy accurately, guided only by the symptoms observed in the healthy: the provings will only give *hints* respecting the relation of a remedy to such a disease.'[1]

[1] *Brit. Jour. of Homœopathy*, vol. x. p. 534.

CHAPTER V.

SECTION I.—ON THE CO-EXISTENCE OF ACUTE INFECTIOUS DISEASES —HOW SIMILAR AND DISSIMILAR DISEASES INFLUENCE ONE ANOTHER, ACCORDING TO HAHNEMANN—HIS ILLUSTRATIONS OF NATURAL HOMŒOPATHY ARE WORTHLESS.

TO avoid frequent repetition, and to enable the reader to appreciate more correctly Hahnemann's opinions regarding the pathological laws which regulate the co-existence and antagonism of diseases, it will be advisable, before proceeding to examine his illustrations of natural homœopathy, to give a summary of what takes place when two or more acute infectious diseases occur in a patient at the same time. We shall confine our remarks to that class of diseases, as he has drawn his illustrations chiefly from it.

'When two morbid poisons co-exist in the same system, their actions are sometimes simultaneous; and each disease runs its course unaffected by the presence of the other. The more usual law of febrile poisons perhaps is, that when two of them co-exist, the one lies latent, while the other runs its course; or they interrupt each other's progress, the active one becoming latent, while the latent one becomes active; and occasionally they modify each other's form and appearance.'[1] 'Morbid poisons co-exist in the same individual, and even produce on some occasions their specific effects on the same membrane at the same time;'[2] and he might have added, almost at the same point of the membrane. Thus Willan informs us (and the truth of his remark has been confirmed by Ring, Bousquet, and others), that 'when vaccine and variolous matter are inserted under the skin so closely together, that as the two pustules enlarge they become one, fluid taken from one side of the common pustule and inoculated will produce

[1] Adams on *Morbid Poisons*, p. 11. [2] *Op. cit.* p. 9.

the vaccine pock, while that taken from the other side will produce a variolous pustule.'[1]

We shall now examine what takes place when two acute infectious diseases appear in the body about the same time.

Small-pox and Measles.—Formerly the co-existence of these two diseases was not uncommon. They may develope themselves simultaneously: measles may precede small-pox, or, what is more frequently the case, the latter may precede the former; measles may suspend the action of small-pox, or, *vice versa*, small-pox may suspend that of measles.

Ring in his work on *Cow-pox*, M'Bride[2] of Dublin, Rainey,[3] Winterbottom,[4] and others, cite numerous cases, in which the two diseases ran their courses simultaneously, without interfering with each other's progress. Willan and Bateman[5] saw two cases in which small-pox and measles co-existed: in the one case, the measles came out on the fifth day of the eruption of small-pox; and in the other, small-pox appeared on the third day of the eruption of measles, which continued visible for two days longer. Pinel, Manget,[6] Rainey,[7] and others, cite many cases in which measles came on several days after the inoculation of small-pox, and suspended the development of the latter until the measles had completed their usual course; and many cases have occurred in which small-pox, supervening during an attack of measles, suspended the latter until the small-pox had completed its course.

Small-pox and Scarlatina.—From the statements of Ring, Williams,[8] Gregory,[9] Marson,[10] Copland,[11] and others, these two diseases have not infrequently co-existed. They have appeared more or less simultaneously. In some cases, scarlatina has

1 *Diseases of London*, by Willan, p. 314.
2 *Practice of Physic*, p. 376.
3 *Edin. Med. Comment.* part iii. p. 480.
4 *Med. and Phys. Jour.* vol. xiv. p. 25.
5 *Edin. Med. and Surg. Jour.* vol. xv. p. 314.
6 *Edin. Med. Comment.* part i. p. 1.
7 *Idem*, part iii. p. 480.
8 *Morbid Poisons*, vol. i. p. 211.
9 *Cyclop. of Pract. Med.* vol. iii. p. 744.
10 *Medico-Chirurg. Trans.* vol. xxx.
11 Copland's *Dict. of Pract. Med.* vol. iii. p. 819.

manifested itself first; in others, small-pox has preceded scarlatina.

Small-pox and Hooping-cough.—Ring, Willan, and others have shown that these two diseases have frequently co-existed, each running its course unaffected by the other. In some cases the hooping-cough made its appearance during an attack of small-pox; in others, the small-pox came on during an attack of hooping-cough. In many cases hooping-cough was immediately superseded by small-pox, the former recurring after the cure of the latter, and continuing its ordinary course. In some cases, however, *the hooping-cough was completely removed by the small-pox,* especially when the former had already lasted some time.[1] The presence of hooping-cough may retard the supervention of small-pox.[2]

Small-pox and Cow-pox.—A great many cases have been referred to by Willan, Ring, and other physicians, in which the two diseases co-existed when the action of their poisons took place in the same individual within a given time. In his work on the *Diseases of London,*[3] Willan says: 'When a person is inoculated with vaccine and variolous matter at the same time, or within a week of each other, both inoculations prove effective, each disease running its usual course without much variation.' He says, however, in his work on *Vaccine Inoculation,*[4] that the vaccine and variolous fluids, inoculated about the same time, do restrain the action of each other on the human body: so that in some cases the vaccine pustule is smaller than usual, and has a very slow progress; in other cases the area is scarcely perceptible; while in others it is large but premature, and the variolous eruption consists of hard, shining pustules, which seldom maturate.

Cow-pox and Chicken-pox.—Many cases are on record in which the two diseases have co-existed, although they frequently modify each other's action.

Cow-pox and Hooping-cough have likewise been seen co-existing, each pursuing its course without being interfered

[1] Mr. Oakes' *Med. and Phys. Jour.* vol. viii. p. 426.
[2] Dessessary, Copland's *Diction.* vol. iii. p. 819.
[3] P. 38.
[4] P. 4.

with by the other; but occasionally the latter disease has been suspended by the supervention of the former, and *even permanently removed by it.*[1]

In all the cases to which I have referred, to illustrate what takes place when the poisons of infectious diseases exist together in the body, two only co-existed; but well-authenticated cases have been cited by Ring, in his interesting work on *Cow-pox,*[2] and by other medical men, in which three acute infectious diseases co-existed. Dr. Hamilton saw six persons in one family labouring under a combination of small-pox, measles, and hooping-cough.

We shall now proceed to examine what Hahnemann has said with regard to the pathological laws which regulate the co-existence and exclusion or antagonism of diseases.

He says: 'The greater strength of the artificial diseases produced by medicines is, however, not the sole cause of their power to cure natural diseases. In order that they may effect a cure, it is before all things requisite that they should be capable of producing in the human body an artificial disease, as similar as possible to the disease to be cured, in order by means of this similarity, conjoined with the somewhat greater strength, to substitute themselves for the natural affection, and thereby deprive the latter of all influence on the vital force. This is so true, that no previously existing disease can be cured even by Nature herself, by the accession of a new *dissimilar* disease, be it ever so strong; and just as little can it be cured by medical treatment with drugs that are incapable of producing a similar morbid condition in the healthy body. In order to illustrate this, we shall consider in three different cases, as well what happens in nature, when two dissimilar natural diseases meet together in one person, as also the result of the ordinary medical treatment of diseases with unsuitable allopathic drugs, which are incapable of producing an artificial morbid condition, similar to the disease to be cured; whereby it will appear that even Nature is unable to remove a dissimilar disease already

[1] Adams, *Morbid Poisons*, p. 12. After noticing Oakes' case (hooping-cough removed by small-pox), he adds: 'The same has frequently happened after vaccination, and I have reason to believe permanently.'

[2] Pp. 107, 1029, etc.

present by one that is unhomœopathic, even though it be stronger; and as little is the unhomœopathic employment of even the strongest medicines ever capable of curing any disease whatever.'[1]

I. If the two dissimilar diseases meeting together in the human being be of equal strength; or still more so, if the older one be the stronger, the new disease will be repelled by the old one from the body, and not allowed to affect it. He illustrates this proposition by saying, that a patient suffering from severe chronic disease will not be infected by a moderate autumnal dysentery or other epidemic. He cites Larrey, to show that the plague does not break out when scurvy is prevalent, and that persons suffering from herpetic eruptions are not affected by it. Hahnemann's illustrations do not seem very appropriate, since the condition mentioned in the proposition, 'two diseases meeting together,' does not exist; and it is not quite clear what he means by the 'stronger disease.' The most natural meaning of the words would be a disease of which the symptoms are more numerous and severe than those of another; but in this sense no one would say that a herpetic eruption, or even scurvy, is a more severe disease than plague. I have already referred to many cases in which two dissimilar diseases attacked a person about the same time, and ran their courses together; and likewise to others, in which persons already affected with one disease were subsequently attacked by another dissimilar one. These facts show that Hahnemann's notions about the antagonism of dissimilar diseases are unfounded.

II. 'Or the new dissimilar disease is the stronger. In this case, the disease under which the patient originally laboured will be kept back and suspended by the occurrence of the stronger one, until the latter shall have run its course and been cured, and then the old one makes its appearance uncured.' He cites several cases, in which two diseases occurring at the same time in the same person, sometimes the one, and sometimes the other suspended each other's progress, as small-pox and measles, cow-pox and scarlatina, measles and cow-pox.

[1] *Organon*, 5th ed. p. 133.

This law holds generally with regard to two acute infectious diseases. He says nothing, however, of the possibility of the co-existence of small-pox and measles, cow-pox and scarlatina, and measles and cow-pox, each disease pursuing its course without being modified by the other, as already shown.

III. 'Or the new disease, after having long acted on the organism, at length joins the old one that is dissimilar to it, and forms with it a complex disease, so that each of them occupies a particular locality in the organism,—namely, the place specially belonging to it.' After stating the third case, in which he says, 'The new disease, after having long acted on the organism, joins the old one,' he illustrates it by the co-existence of syphilis and scabies; but a little further on he rather inconsistently refers to several cases in which two *acute* infectious diseases co-existed. As we have already shown, such cases have occurred frequently enough, much more so than Hahnemann seems disposed to admit.

'Totally different, however, is the result when two similar diseases meet together in the organism, that is to say, when to the disease already present a stronger similar one is added. In such cases we see how a cure can be effected by the operations of nature, and we get a lesson as to how we ought to cure.' No! invariably and in every case do two diseases differing certainly in kind, but very similar in their phenomena and effects, and in the sufferings and symptoms they severally produce, annihilate one another whenever they meet together in the organism: the stronger disease, namely, annihilates the weaker; and that for this simple reason, because the stronger morbific potency, when it appears, does, on account of its similarity of action, involve exactly the same parts of the organism that were hitherto affected by the weaker morbid irritation, which consequently can no longer influence the system, but is extinguished.

Let us examine the cases which he brings forward as homœopathic cures made by Nature herself.

1. [1] 'How frequently does small-pox produce violent ophthalmia, terminating even in blindness! See! by its inoculation, Dezoteux cured a chronic ophthalmia permanently, and Leroy

[1] *Op. cit.* p. 147, etc.

another.' The first case which he has selected as an illustration of natural homœopathy, shows what we have already pointed out, how vaguely the word 'like' is used, and how arbitrarily it is applied. Surely no supporter of homœopathy will pretend to find a homœopathic similarity between the symptoms of chronic ophthalmia and small-pox. We might as well say that there is a homœopathic similarity between chronic bronchitis and measles, and conclude that, because acute bronchitis is a constant result of the pathogenesis of measles, the latter can therefore cure chronic bronchitis; or that the symptoms of chronic angina faucum resemble those of scarlatina, because the latter produces acute angina, and must therefore cure chronic angina. Instead of seeking to account for the cure of the two cases of chronic ophthalmia by a supposed resemblance between it and small-pox, it would be more reasonable to ascribe it to the influence of the extensive inflammation and suppuration of the skin produced by inoculation.

2. 'An amaurosis of two years' duration, consequent on suppressed tinea, was perfectly cured by it, according to Klein.' Amaurosis cannot be regarded as a pathogenetic effect of small-pox; and the cure, therefore, cannot be explained on the homœopathic principle, *similia similibus.*

3. 'How often does not small-pox cause deafness and dyspnœa! And both these chronic diseases it removed on reaching its acme, as J. Closs observed.' Dyspnœa and deafness are merely symptoms of disease, and it would be absurd to talk of tracing a homœopathic resemblance between a symptom and a disease.

4. 'Swelling of the testicle, even of a very severe character, is a frequent symptom of small-pox; and on this account it was enabled, as Klein observed, to cure, by virtue of similarity, a large hard swelling of the testicle that had arisen from a bruise.'[1] Swelling of the testicle is certainly not a pathogenetic effect of small-pox, and consequently the homœopathic law cannot be employed to explain the cure. That a chronic swelling of the testicle disappeared after an attack of small-pox

[1] Hahnemann made a mistake in speaking of two cases of enlarged testicle. There was only one case related by Klein; but the same case was published in two different works.

I can well imagine; but the result was not owing certainly to a homœopathic similarity between the two diseases. It could be more satisfactorily accounted for by the extensive inflammation and suppuration of the skin caused by small-pox. The same explanation can be given of the two preceding cures, Nos. 2 and 3.

5. 'The inoculated cow-pox, whose lymph, besides the protective matter, contains the contagion of a general cutaneous eruption of another nature, consisting of usually small dry (rarely large pustular) pimples, resting on a small red areola, frequently conjoined with red cutaneous spots, and often accompanied by the most violent itching, which rash appears in not a few children several days before—more frequently, however, after the red areola of the cow-pox—and goes off in a few days, leaving behind small, red, hard spots on the skin;—the inoculated cow-pox, I say, after it has taken, cures perfectly and permanently, in a homœopathic manner, by the similarity of this accessory miasm, analogous cutaneous eruptions of children, often of very long standing, and of a very troublesome character.' A trifling eruption, generally of a papular, but sometimes of a vesicular form, which goes off in a few days, not unfrequently accompanies vaccination. As Hahnemann does not specify what analogous eruptions were removed, we cannot judge of their resemblance to that which sometimes accompanies vaccination. He says, however, they were often of very long standing, and of a very troublesome character; and in these respects they were quite different from the vaccine eruption.

6. 'The cow-pox, a peculiar symptom of which is to cause tumefaction of the arm, cured, after it broke out, a swollen, half-paralyzed arm.' We have here an example of the want of accuracy in his quotations, for which Hahnemann was rather notorious.[1] The disease of which he speaks (it continued about three years) commenced with pain in the right side, which afterwards extended upwards to the right side of the neck, and to the whole of the right arm: its sensibility became impaired, and its contractile power was much diminished. The most striking symptom of the complaint was constant, and often very

[1] Duncan's *Annals of Medicine*, Lust ii. vol. i. pt. 2.

severe, pain in the parts affected. The disease was evidently connected with the state of the nerves distributed to the affected parts, and the physician who treated it took that view of it. It was completely cured by vaccination; but there was not the slightest resemblance between the symptoms of the disease and the effects of vaccination. The application of the principle *similia similibus* has been sadly abused by Hahnemann.

7. 'The fever accompanying cow-pox, which occurs at the time of the production of the red areola, cured homœopathically an intermittent fever in two individuals, as the younger Hardege reports, confirming what Hunter had already observed, that two fevers cannot co-exist in the same body.' Again Hahnemann has misstated the case to which he refers. Hardege says nothing about intermittent fever. After affirming that febrile complaints were not aggravated by vaccination, he briefly observes: 'In two fever patients the fever became less, and soon disappeared after the appearance of the rose-coloured inflammation.'[1] In an extract which we have given from the *Organon*, Hahnemann himself refers to cases, when illustrating his pathological laws, which completely upset Hunter's opinion.

8. 'The measles bears a strong resemblance in the character of its fever and cough to the hooping-cough; and hence it was that Bosquillon noticed in an epidemic, where both these affections prevailed, that many children who took measles remained free from hooping-cough during that epidemic. They would all have been protected from, and rendered incapable of, being infected by the hooping-cough in that and all subsequent epidemics by the measles, if the hooping-cough were not a disease that has only a partial similarity to the measles, that is to say, if it had also a cutaneous eruption similar to what the latter possesses.' Measles has never produced hooping-cough, consequently it cannot influence that complaint according to the homœopathic law; and Hahnemann says there is only a *partial* similarity between the two diseases.

9. 'If, however, the measles come in contact with a disease resembling it in its chief symptom—the eruption—it can indisputably remove and effect a homœopathic cure of the latter.

[1] Hufeland's *Journal f. prakt. Heilk.* vol. xxiii. p. 147.

Thus a chronic herpetic cruption was entircly and permanently (homœopathieally) cured by the brcaking out of thc measles.' In several important respects these two diseascs—measlcs and chronie herpes—prcsent, instead of resemblanee, a markcd .eontrast with eaeh other. Measles is an acute infcctious disease of definite duration, accompanied with fever and general maeular eruption of the skin; seldom attaeks an individual more than onee. Chronie herpes is not infeetious, of indefinite duration, and seldom aeeompanied with fever; the eruption is vesieular, and limited to a eertain portion of the skin: it may attaek the same person frequently. The prineiple *similia similibus* is very elastie, and can be easily adapted to any amount of similarity.

The last illustration to be examined is the most important that Hahnemann has brought forward in favour of natural homœopathy; for it is the only one in whieh a similarity can be admitted to exist between the two diseases.

10. 'Small-pox eoming on after vaeeination, as well on aceount of its greater strength as its great similarity, immediately removes entirely the eow-pox homœopathieally, and does not permit it to eome to maturity; but, on the other hand, the cow-pox, when near maturity, does, on aeeount of its great similarity, homœopathieally diminish very mueh the supervening small-pox, and make it mueh milder.' Let us see whether or not this statement is in aeeordance with faets.

At page 3 of his work on *Vaccinc Inoculation*, Dr. Willan repeats the statements which he had formerly made in his work on the *Discases of London*: 'That when a person was inoculated with vaeeine and variolous matter about the same time, both inoeulations proved effeetive, for the vaeeine vesicle proeeeded to its aeme in the usual number of days, and the maturation of the variolous pustule was attended with a pustular cruption on the skin; that these effeets took place without mueh variation in all eases where the interval between the two inoculations did not exceed a week; that when variolous matter was inserted on the ninth day after the vaceine inoculation, its aetion seemed to be wholly precluded.' In the same work (*Vac. Inoc.* p. 7) he answers a question subjoined to his formcr one: 'Do the variolous and vaccine virus, under thc circumstances mentioned, act independently, or do they control each other's operation?' The conclusion to be drawn from my more extensive experience is, 'that variolous and vaccine virus, inoculated at the same time, restrain the operation of each other in the body, and somewhat alter the form of the pustules or vesicles, without effecting any change in the qualities of the fluid they contain.' 'I was fully satisfied that these modified pustules were genuine variolous pustules, as I had many opportunities of ascertaining by inoculation that they were capable of communicating every species of small-pox, from the mild and distinct to the confluent and fatal; and fluid taken from the vaccine vesicle on the arm of the person affected with the variolous fever and eruption, and inserted into the arm of another person by a clean lancet, produced the vaccine disease alone.'

The conclusions of Dr. Willan have been confirmed by M. Bousquet in his able work on *Cow-pox*, in which he states, as the result of his observation—1*st*, That when small-pox and cow-pox appear together at the same time, or within two or three days of each other, each follows its own natural progress uninfluenced by the other, and that whether the cow-pox or small-pox appears the first. 2*d*, The case is different when either of these two eruptions has a longer period of precedence of the other; for then the disease which appears last, whether it be small-pox or cow-pox, is modified, and its course shortened; whilst the affection which had first broken out keeps its vantage-ground, and terminates at the usual time, without having undergone the slightest change either in its form or in its duration.

I may add that many cases have been observed by Ring and others, in which the natural small-pox co-existed with the vaccine pustule, without essentially modifying (even when the former was of a very virulent nature) the development of the latter. Mr. Marson, of the Small-pox Hospital, says: 'I have several times seen small-pox and vaccine disease advancing *pari passu*, without the usual progress of each disease being respectively interrupted.' Again, many well-established cases have occurred in which a variolous pustule formed so near the vaccine one, that, as they enlarged, the two coalesced and formed one. In children inoculated with matter taken from the side of the vaccine pustule, cow-pox resulted, and children inoculated with matter from the side of the variolous one got small-pox.

The facts that have been stated while examining the action of cow-pox and small-pox, clearly prove that Hahnemann's opinion regarding the homœopathic removal of the former by the latter is quite erroneous. It must be admitted that he has been very unsuccessful in his attempt to illustrate natural homœopathy; and after reading what he has written on the subject, one can feel the force of the remark of Griesselich, one of the ablest homœopathic physicians of his day: 'These examples of *homœopathia involuntaria* place Hahnemann's judgment, or his love of truth, in no favourable light.'

SECTION II.—ACCORDING TO HAHNEMANN, NO DISEASE CAN BE CURED UNLESS BY PRODUCING A NEW ONE—HE SCARCELY ADMITTED THE POSSIBILITY OF SPONTANEOUS CURES—HIS EXPLANATION HOW DRUGS CURE DISEASE.

In Hahnemann's remarks on the phenomena presented by the co-existence of two diseases in the same body, we found many inaccuracies, and one or two important errors. He said that dissimilar disorders do not cure one another; and with one or two exceptions, perhaps, his remark is true, as far as regards acute infectious ones; but in the only satisfactory case of similarity—that between cow-pox and small-pox—which he has brought forward, his opinion that a disease will be annihilated by a similar stronger one was shown to be unfounded. In short, in the cases of two similar as well as of two dissimilar diseases, no cure was made, with the exception of those of small-pox and hooping-cough, and of cow-pox and hooping-cough, in which one acute infectious disorder was removed by another. In the present state of pathology it is impossible to explain why this should have occurred only in the cases to which I have referred. It is remarkable enough, that these cures took place in *direct contradiction* to Hahnemann's opinion, that dissimilar maladies cannot remove one another.

But the truth is, Hahnemann has entirely misinterpreted nature by the false view which he took of the phenomena observed, when two diseases occur at the same time in the body. Their co-existence certainly presents important pathological phenomena, but it is quite an erroneous idea to suppose that in these cases nature is attempting to cure one disease by means of another. As we formerly remarked, the pathological law, in cases in which two infectious maladies co-exist, is, that in the majority of them the two modify more or less each other's action; but there is no reason to suppose that these phenomena indicate an effort of nature to cure one by means of another. Were that the case, Hahnemann might have spoken with propriety of the 'pitiable and highly imperfect efforts of the vital force to relieve itself from disease.' In these cases, however, he overlooked a very natural and important question: What removed the two diseases when they ran their courses simultaneously? and when the course of the one was suspended by the other, what removed the disease that progressed, and subsequently the other, when it resumed its course?

Although possessed of great power of observation, Hahnemann was very apt to look at nature through the deceptive medium of theory. He thought no disease could be cured except through the influence of another similar one; and he spoke with contempt of what he considered the efforts of Nature to cure disease. He scarcely admitted the possibility of natural cures, even of the simplest and mildest diseases: 'It is undeniable that our vital force is unable, without the assistance of true curative agents, administered by human skill, to combat even inconsiderable acute disease, if even it do not succumb to them.'

Hahnemann's works are remarkable for the number of contradictory opinions to be found in them. Although in general he speaks unfavourably of the power of Nature to cure disease, yet occasionally, according to circumstances, he accords a more important part to her in its removal: 'That kind nature and youth will, assisted by such an appropriate regimen (for it is nothing more), and even by itself, cure diseases having far other producing causes than excess and deficiency of excitability, is a phenomenon daily witnessed by the unprejudiced observer.'[1] And again, in a passage already quoted: 'In such cases we see how a cure can be effected by the operations of nature, and we get a lesson as to how we ought to cure.'[2]

In general, however, he seemed to ignore the fact so well established by the observation of medical men, that every possible variety of disease (with the exception, perhaps, of organic), from the slightest to the most formidable, from a cold in the head to pneumonia, typhus fever, and even cholera, can be cured by the resources of nature, unaided by medicine. The medical literature of every age and country abounds in facts which confirm the truth of my statement; and every medical man who has had considerable practice, especially in countries where medical aid could not be easily procured, must have frequently seen and heard of cases, even of dangerous diseases, which recovered without the use of medicine. We daily see the slighter cases of catarrh, bronchitis, dyspepsia, diarrhœa, rheumatism, and many other affections, recover without medicinal aid. During the last twenty or thirty years, trials have been made on a large scale in various hospitals, in order to ascertain how much nature, unaided by medicine, can accomplish in the cure of such diseases as rheumatic and typhoid fevers, pneumonia, etc. The result has generally been more favourable than when they were treated according to the plans formerly recommended in medical works.

From what has been observed in the natural progress of disease, I think the following conclusions can be drawn:—1. When disease is slight, or, in other words, when the deviation from the healthy state is not great, there is a tendency in the diseased parts to return, more or less gradually, to the healthy or normal state. 2. When disease is more severe, or the deviation from the normal state greater, the tendency of diseased parts to return to the healthy state is less marked, and the use of remedial measures becomes necessary to promote and accomplish that object. 3. When disease is severe, or when the deviation from the healthy state is very great, the tendency may

[1] Hahnemann's *Lesser Writings*, p. 413.
[2] *Organon*, p. 145.

be more towards dissolution or death; and in these cases remedial means have comparatively little control over its progress.

It happens, however, in some cases, that nearly the same apparent amount of disease may give rise to very different results from those which usually occur. Thus a slight disorder, which generally terminates in health, may assume a more severe form; new morbid states (complications) may arise, and the case may ultimately terminate fatally; or a diseased state, which generally terminates in death, may unexpectedly begin to improve, and ultimately be entirely removed. Notwithstanding these exceptional cases, we may regard it as a pathological law, that the tendency of diseased parts to return to their healthy state is inversely as the amount of their deviation from it.

When an acute disorder has reached its culminating point, the affected parts begin to return more or less gradually to their healthy state. The order in which the amelioration takes place varies very much in different cases, even of the same morbid state, which may continue for a time to increase in one point of the parts affected, whilst it is diminishing in another. In the great majority of cases, however, *it is neither preceded nor accompanied by the development of a new disease;* and this fact completely upsets the opinion of Hahnemann and other physicians, who believe that no malady can be cured except by the supervention of another natural, or of a medicinal one.

In a long, very confused, and intricate sentence, he gives us his theory of the action of medicine on disease, to which, however, he informs us he does not attach much importance, although he considers it a highly probable explanation of the process: 'As every disease not strictly surgical depends only on a peculiar morbid derangement of our vital force in sensations and functions, when a homœopathic cure of the vital force deranged by the natural disease is accomplished by the administration of a medicinal potency, selected on account of an accurate similarity of symptoms, a somewhat stronger but similar artificial morbid affection is brought into contact with, and as it were pushed into the place of, the weaker similar natural morbid irritation, against which the instinctive vital force now merely (though in a stronger degree) medicinally diseased, is then compelled to direct an increased amount of energy; but on account of the shorter duration of the action of the medicinal potency that now morbidly affects it, the vital force soon overcomes this; and as it was in the first instance relieved from the material morbid affection, so it is now at last freed from the artificial (the medicinal) one, and hence is enabled to carry on healthily the vital operations of the organism.' [1]

Hahnemann has just stated—and he does so in many parts of the *Organon*—that the natural disease is produced by the previous derangement of the vital power; so that by the phrase 'vital force deranged,' etc., he makes the former at once an effect and a cause of derangement of the latter. 'Brought in contact with, and as it were pushed into the place of, the weaker.' Here he commits the incredible absurdity of speaking of disease as a concrete something—as an entity—in order to make his explanation clearer, 'as it were,' but without enabling us to imagine how the medicinal disease can be pushed into the place of the natural one.

The attempts made by homœopathic writers, since the first publication of Hahnemann's system, to explain the action of medicines, though they equal in worthlessness, far surpass in number, those made by practitioners of the old school during that period. However much the explanations of some of them may differ from that of Hahnemann, they generally admit the production of a disease similar to the natural one by the drug employed in the treatment; so that, theoretically, they must make the absurd attempt to fancy the simultaneous existence of two similar morbid states in the same, or according to some, in the neighbouring parts. Thus, in a case of pneumonia treated with phosphorus, they must conjure up the existence of a second inflammatory state (from the action of phosphorus) in the parts already inflamed. The only conceivable effect of a drug capable of producing in a healthy person a disease more or less similar to the natural one already existing, would be to intensify that diseased state, or, in other words, to aggravate the disease. Thus, Roberts saw a case of tetanus, in which strychnia (which produces, when given to a healthy person in

[1] *Organon*, p. 127.

sufficiently large doses, a train of symptoms very like those of that disease), administered in small medicinal doses, caused frightful aggravation of the symptoms. One of his friends, Mr. Reeve, likewise tried it in another case of tetanus; but the paroxysms became so violent under its influence, 'that the patient was actually projected from her bed.'

Even supposing the dose of strychnia so much diminished as to produce only the slightest possible effect on the nervous system, it would still cause, *pro tanto*, an aggravation of the symptoms. The author of an article in the number of the *British Journal of Homœopathy* for April 1864, p. 290, says: 'It is obvious that, if a drug is capable of acting upon the same tissues as those involved in disease, and in a manner similar to that of the disease, it must be always possible, by giving a sufficiently large dose, to produce an aggravation of the symptoms.' 'But it seems also to follow, that though you may so far reduce your dose that no aggravation is apparent, it may yet be sufficiently large to keep up the diseased action.' At p. 397 of the same journal for 1862, Mr. Gelstone says: 'We know for certain, that medicines whose action is very similar, when exhibited in quantities sufficient to evoke appreciable pathological effects, exalt each other's influence.'

But to return to the explanation of the action of drugs given by Hahnemann. He leaves it to his readers to imagine for themselves in what way 'the vital force is then compelled to direct an increased amount of energy.' But for what purpose this increased energy, since by theory the natural disease has already been displaced? The natural disease once displaced or removed, Hahnemann disposed very easily of the stronger medicinal one. 'On account of the shorter duration of the drug disease,' he says, 'the vital force soon overcomes it.' But if, as the theory says, the drug disease must be a little stronger than the natural one; or, to recur to our former illustration, if phosphorus produces a stronger inflammation of the lungs than that already existing, we may presume that the morbid state caused by the medicine will be of as long duration, at least, as that of the natural disease. It has been pertinently remarked by homœopathists as well as by allopathists, that if the vital force can overcome the theoretically stronger, it should be still more able to remove the weaker natural disease. This objection would be sufficient of itself to overthrow Hahnemann's theory of the action of medicines.[1]

[1] Allopathy does not appear to be more fortunate than homœopathy in her attempts to explain the action of drugs. In a work which has acquired considerable reputation, the author explains the action of sedative medicines in the following manner: 'and state my belief in the bare possibility of the operation of neurotic agents being explicable on mechanical grounds. It is generally believed among scientific men, that each particle of a compound body is made up of a number of indivisible atoms, each of which is inconceivably minute in size. And as these compound bodies have each a peculiar chemical constitution, so must each of their ultimate parts be composed of a peculiar arrangement of simpler atoms, and thus have a certain shape of its own, more or less different from the shape of every other compound atom. Both the substance of a nerve, and the active part of a nerve medicine, consist of a number of definite compound atoms. And it is possible that the atom of a stimulant medicine may be of such a shape as that it shall be unable to coincide with, or to fit into, the series of atoms forming the sensitive surface of the nerve, and thus irritate this when brought in contact with it; and that the compound atoms of a sedative may so arrange with these nerve particles, as to fit among and extinguish the salient points, and annihilate their natural sensibility. But even if the theory of the action by atomic shapes should be rejected as improbable, because affording *too easy an explanation* of a naturally inscrutable operation, it would still seem likely that these medicines may take effect by exerting some minute and imperceptible influence on nerve fibre, or producing in it some inappreciable disorganization, or change, which has the effect of altering the natural performance of its function.'—Headland on the *Action of Medicines*, p. 248. It is true the author, like Hahnemann, professes not to attach much importance to his explanation of the action of medicines; but if he did not believe it had some pretensions to truth, why did he publish it? Of Hahnemann's and Headland's hypotheses of the action of medicine, we may say, 'les deux font la paire.'

CHAPTER VI.

WHAT HOMŒOPATHISTS HAVE THOUGHT OF THE PRACTICAL APPLICATION OF THEIR THERAPEUTIC PRINCIPLE—PROPOSED MODIFICATIONS OF THEIR DOCTRINE.

BEFORE concluding our examination of the principle *similia similibus curantur*, it will be interesting to know what homœopathic writers have thought of its practical application. As we formerly stated, Hahnemann considered the totality of the symptoms as the disease, and the sole indication for the choice of a medicine. He likewise said truly that no two diseases are exactly alike, and maintained that the great aim of physicians should be to individualize disease. But if this rule were rigidly carried out, and if unimportant differences between similar diseased states necessarily involved a modification of their treatment, generalization would be impossible, and experience would become a word without meaning.

When observing homœopathic practice at the bedside, I have not infrequently remarked that the practitioner, instead of individualizing, generalized, and referred to his past experience for the choice of a remedy which he had found beneficial in cases similar to the one before him. In the following quotations it will be seen that, in many cases, homœopathic practitioners attach great importance to experience as a guide in the choice of a remedy: in fact, they admit that in many cases it is indispensable, and consequently, that the principle *similia similibus* is an insufficient guide in the treatment of disease.

Dr. Kopp says: 'The choice of a remedy according to the symptoms seems often as unsatisfactory as in the allopathic system.' 'In practice, the principle of homœopathy not infrequently disappoints us. The most suitable remedy, employed after mature reflection, and having the greatest resemblance to the illness, does not bring about the desired result. Thus, a remedy is prescribed in a case of intermittent fever without a satisfactory result; another is then selected suited to the symptoms; and so on, till the whole series of remedies against intermittent fever is tried, as often happens in allopathic practice. At last it is said, *Es liegt innere Psora zu Grunde,* although no traces of the existence of scabies could be found during the patient's whole life.'[1] Hartlaub says: 'It is often extremely difficult to find the proper remedy for each case of intermittent fever, and one must have treated a great many cases before one can proceed with anything like certainty. One must have learnt from *experience* what this or that remedy can do: a mere string of symptoms taken from the Materia Medica, and not from nature, is worthless as a guide in practice.'[2]

Wurmb says: 'They may not do it so rapidly, for the remedies suited to the different states of intermittent fever do not lie before us, but must often be long searched for, until the proper one be found.' 'Much oftener we ourselves were in fault, when the paroxysms returned so often, as from following the old-established plan in choosing a remedy: till *experience* taught us a better, we did not at once hit upon the right remedy.' 'In the treatment of our cases of intermittent fever we chose a remedy 154 times: 77 times with good results, as we found the proper remedy; 77 times our choice was unfortunate.' 'The inefficiency of aconite in this complaint confirms our views with regard to the choice of a remedy, and gives a proof that the symptoms alone are not quite a certain guide (in the choice of a remedy), but must be taken in conjunction with proper consideration of the nature of the complaint.'[3]

Dr. Trinks remarks: 'The cause of this is, that the greatest possible similarity between the effects of the medicine and those of the disease is sufficient in a great many cases (to guide us in the choice of a proper remedy); in others, only *partially;* and in many, *not at all.* It is possible that a more exact proving of medicines than we yet possess will enable us to choose the proper medicine with greater certainty.'[4] Hempel

[1] *Denkwürdigkeiten aus der Praxis*, vol. iii. p. 264.
[2] *Annalen der hom. Klinik*, vol. iii. p. 376.
[3] *Homœop. klin. Studien*, p. 128.
[4] *Vierteljahrsschrift*, vol. ii. p. 303.

says: 'The result is this discouraging conclusion, that the homœopathic law of "like cures like" is only an apparent truth, and therefore in many cases without any practical value.'[1] Dr. Elbe: 'The determining grounds for the choice of the several remedies given in the following pages are derived more from *experience than from the symptoms developed by the provings.* But it is impossible, in a disease conjoined with material changes (scarlatina), to choose the remedy accurately, guided only by the symptoms observed in the healthy: the provings will only give hints respecting the relation of a remedy to such a disease.'[2]

Dr. Dudgeon says: 'However convinced we may be of the theoretical truth of the homœopathic law, its practical application is by no means always easy. The pathogeneses of the Materia Medica sometimes afford us but the vaguest indication for our selection of a drug; sometimes many medicines will appear to offer a closer correspondence to the case before us than the one which ultimately proves to be the suitable one. Again, the disease may be of such a sort, that there cannot be anything like an analogue to it in our repertory of medicinal diseases; for our provings cannot be carried to the production of serious maladies. In such cases, a good deal of the vaunted mathematical certainty of homœopathy is but guesswork, and as such is very apt to be unsuccessful. *Clinical observation, the* usus in morbis, *which Hahnemann denounced, but availed himself of largely, is what we must look to, to enable us to prescribe with certainty in almost every case, but especially in such as I have alluded to.*'[3] 'And the discouraging thought has often struck me, if the knowledge of the relations of our therapeutic agents to the varieties of ophthalmia, where the symptoms are mostly objective and easily recognisable, be so vague and unsatisfactory, how much more so must be our knowledge of their relations to other diseases, where the symptoms are mostly subjective and uncertain!'[4]

Dr. Black says: 'It may be viewed as a difficult task for

1 *Organon of Spec. Homœop.* p. 117.
2 *Brit. Jour. of Homœop.* vol. x. p. 534.
3 *Idem*, vol. xiii. p. 134.
4 *Idem*, vol. vii. p. 1.

one who refers to these records of symptoms (Materia Medica), to select the right remedy. Less embarrassment, however, occurs in practice; because, after *years of experience at the bed-side*, homœopathists become gradually acquainted with the characteristics of each (medicine), its total operation and particular tendencies.'[1]

Hartmann admits: 'It not infrequently happens in acute diseases, that even the medicines specifically suited to the cases do not show their full action, but cause only some unimportant changes, and even leave the disease quite untouched.'[2]

In a late edition of his ably written work on *Homœopathy*, Hirschel makes the following remarks: 'The utility of iodine, bromium, spongia, etc., in croup is known; and when we compare the symptoms which they produce with those of the disease, we find a great resemblance between them. But who will deny that many other drugs, such as bryonia, sepia, ipecacuanha, etc., produce very similar, if not quite such characteristic symptoms, although *experience* has not shown that they possess any specific relation to the disease?'[3] 'Besides, every practitioner will be able to convince himself, from his own experience, that symptomatic resemblance alone, however striking it may be in appearance, between the action of a medicine and a disease, would often disappoint us if other considerations derived from *observation and reflection* did not enable us to make the proper choice from amongst several apparently suitable remedies.'[4]

In an account which he gives of a severe epidemic of scarlet fever, Dr. Elbe remarks, that rhus appeared to be amongst the most useful (remedies): 'Nevertheless, rhus has no particular specific relation to scarlet fever; and bryonia and phosphorus, which corresponded best to the diseased state, did nothing at all.' Of belladonna he says: 'In general, I did not find the prophylactic power of belladonna by any means so generally borne out, although cases have come before me in which I gave

[1] *Principles of Homœopathy*, p. 60.
[2] *Archiv. f. d. hom. Heilk.* vol. viii.
[3] *Compendium der Homœopathie*, p. 111.
[4] *Idem*, p. 95.

it as a preventative; and the children to whom I administered it remained free from scarlet fever. But just as often I have found that children have been attacked by it, notwithstanding the use of belladonna for several weeks, and that this long previous use of it had not even the power of diminishing the violence of the disease.'[1] In an article by Dr. Wylde, on *Belladonna as a Prophylactic*, he says that, 'according to the experience of Drs. Lehmann and Elbe (whose experience is confirmed, I believe, by many other homœopathists), belladonna would appear to have little power either as a preventative or as a curative in the malignant forms of scarlatina. In speaking on this subject with my brother homœopathists, I found more scepticism than faith with regard to the powers of belladonna as a preventative in scarlet fever.'[2]

Dr. Fleischmann relates as follows, in a volume of the *Hygea*: 'I formerly employed aconite, bryonia, cannabis, etc., in inflammation of the lungs, and I was pretty successful in my treatment; but each of these remedies should be used only in the cases suited to it. I have had the greatest difficulty to find the suitable remedy in each individual case; and after the cure, I have found it difficult to say to which of the remedies it was owing. Since I have used phosphorus, I find these difficulties no longer.'[3] A distinguished supporter of homœopathy, Dr. Arnold, says: 'According to the law *similia similibus*, we cannot expect any benefit from the use of phosphorus in real pneumonia.'[4] And Dr. Wurmb supports his view: 'In a word, phosphorus causes an alteration of the state of the blood exactly the opposite of the fibrinous, and it cannot therefore be the proper homœopathic remedy for fibrinous pneumonia. I cannot understand how phosphorus can suit at one time typhoid, at another croupal pneumonia; why in one case it augments, and in another diminishes, the plasticity of the blood: in short, how one and the same remedy can be the ὅμοιον, the homœopathic remedy, in essentially different diseases. Dr. Fleischmann must be able to show that the phosphorus provings,

[1] *British Jour. of Homœop.* vol. vii. p. 35.

[2] *Idem*, vol. xv. p. 7.

[3] *Hygea*, vol. viii. p. 329.

[4] *Hom. Vierteljahrsschrift*, vol. i. p. 156.

including Hahnemann's, are worthless, and that the remedy works very differently from what they teach.'[1]

Again, Dr. Fleischmann considered arsenic very efficacious in *typhus abdominalis.* Hausmann thought it even a specific for that disease; but Dr. Wurmb would not admit its efficacy. 'Arsenic,' he says, 'corresponds to some symptoms of typhus, but it can never be the homœopathic remedy of that disease. The typhus process has no similarity to the action of arsenic. Our experience quite contradicts that of our friend Fleischmann, who has had much better results since he has used almost exclusively arsenic in the treatment of *typhus abdominalis.* We have used it in various dilutions in that complaint. Patients have recovered while using it; but it produced no change in the progress of the disease. The patients recovered, but arsenic did *not cure them.*'[2] Dr. Dudgeon was struck by the discrepancy of the homœopathic treatment of variola: 'One physician spoke of one medicine never having failed him; another found no effect from the same remedy, but had found some other efficacious.'[3] Bönnighausen, who insisted so much on the importance of a proper similarity between the symptoms of the medicinal and natural disease, spoke of the 200th dilution of thuja as an infallible remedy in small-pox, and affirmed that its administration would supersede altogether the necessity for vaccination. Dr. Trinks asserted, on the contrary, that during the epidemic at Dresden, neither thuja nor tartar emetic was of the slightest use: vaccinine and varioline did good.

We shall finish our quotations from homœopathic writers by giving an extract from Rapou's *History of the Homœopathic Doctrine,* which will show that homœopathic practitioners can occasionally generalize as freely as those of the old school. 'At Prague,' he informs us, 'Dr. Hirsch remarked the influence of the medical constitution. Remedies which cured most successfully at one time, failed completely in the same complaint at another. At Prague, rhus suited the rheumatisms of last year; but it is useless in those occurring this year, although the character of the complaint is the same. In rheumatism, at Linz,

[1] *Homœop. klinisch. Studien,* p. 76.
[2] *Œsterreich. Zeitschr. f. Homœop.* vol. i. Hft. 3, p. 100.
[3] *Brit. Jour. of Hom.* vol. ix. p. 503.

up to the present time, rhus has no effect. At Prague, the veratrum album was very efficacious in hooping-cough last year; at present it is the ambra grisea.' 'It is there, at Vienna,' the father of Rapou writes, 'I learned that certain diseases are really modified by the climate in which they are produced; and although presenting apparently the same symptoms, they cannot be cured by the same remedies. For example, certain forms of intermittent fever, which are removed in Saxony by means of china, nux vomica, and pulsatilla, in Hungary and other parts of Austria require ipecacuanha, ignatia, chamomilla; whilst at Berlin sepia is the most efficacious remedy against this complaint. Certain chronic diseases of the skin, which in Vienna yield easily to the use of graphites, dulcamara, tinct. of sulphur, are cured at Pesth with lycopodium, carb. animalis, and arsenic; whilst at Dresden they require the use of sarsaparilla, natr. muriaticum, rhus, conium.'[1]

If the remarks contained in the extract just given are true, they must tend to shake the faith even of the most stedfast homœopathist in the principle *similia similibus.* If not, they will tend to throw discredit on the accuracy of a number of homœopathic observers; while they prove, at the same time, that the principle *similia similibus* is but an uncertain guide in the treatment of disease.

In the quotations which we have given from homœopathic writers, and to which we could easily add many similar, it is remarkable that, while admitting the insufficiency of the symptoms alone to guide practitioners in the choice of a remedy, they generally refer to *experience* as a means for supplying the deficiency. Even Hahnemann himself, notwithstanding his frequently repeated statement, that the symptoms alone are the only proper guides in the choice of a medicine, and his frequent condemnation of experience, or, as the homœopathists usually term it, *usus in morbis*, as a guide in the treatment of disease, has drawn much of his knowledge of the properties of drugs from that source; indeed, the greater part of the drug symptoms described in his work on chronic diseases are derived from it. With his characteristic inconsistency, however, he occasionally boldly generalizes, espe-

[1] *Histoire de la Doctrine Homœopath.* vol. ii. p. 11.

cially in his later works, and recommends medicines of which *experience* had shown him the utility in many different morbid states.

'Nux vomica,' he remarks, 'is peculiarly adapted to persons of an anxious, irritable, choleric disposition, while pulsatilla suits particularly those of a mild, timid, desponding one. Nux vomica is useful in affections occasioned by the abuse of wine or coffee; pulsatilla, when the stomach has been deranged by the use of fat food, particularly pork; crude antimony, when the derangement is with eructations having the taste of the food; smelling of arsenic, when from eating fruit. In cases of recent fright, especially if it has caused fear, opium is indicated; but when a considerable time has elapsed since its occurrence, or when combined with vexation, aconite; if grief be the effect of fright, ignatia; vexation with febrile symptoms, bryonia.' In no two cases of each of these different morbid states could the symptoms be exactly alike; so that, practically, Hahnemann did not seem to attach much importance to considerable differences in the symptoms of different cases of similar morbid states. But what is most remarkable is his occasional admission of what he had learnt from *experience;* and it must be confessed that the pathogeneses of most of the remedies which we have just noticed would not have indicated the uses for which he has recommended them,—a proof of the insufficiency of the principle *similia similibus* as a guide in practice.

In the earlier days of homœopathy, when the totality of the symptoms was the only guide in the treatment of disease, its practitioners made the tracing of resemblance between the symptoms of medicinal and natural diseases an almost mechanical operation (not a few, even at the present day, follow almost the same plan), to which a term with a special meaning was applied, 'covering' the symptoms. Many serious objections, however, were made to this practice, amongst the most important of which was the well-established fact, that very different morbid states, especially of the nervous system, may give rise to very similar symptoms. The force of the objections was admitted, and different writers suggested different plans to obviate the defects of the guiding principle. Some of them advised that the characteristics of the natural and medicinal diseases should be considered, rather than the similarity of their symptoms, without stating in what the characteristics consisted; others showed that Hahnemann had called attention to the exciting causes of disease. In no system of therapeutics can the importance of endeavouring to ascertain, and if possible to remove them, be overlooked; but the pathogeneses of drugs give us no information about exciting causes, most frequently external agents, or the means for removing them. That information must be drawn from other sources—observation and experiment.

Since Hahnemann's death, a more decided renunciation of some of his tenets has taken place. The psora and dynamization hypotheses have been gradually losing ground, and at present olfaction is seldom, if ever employed. Many practitioners have spoken in favour of experience as a source of therapeutic knowledge,—a circumstance which indicates a disposition to adopt more rational views of practical medicine. To admit, however, the utility of experience, implies a tacit recognition of the insufficiency of the therapeutic law *similia similibus*. A still greater number have recommended that ætiology, semiology with diagnosis, and morbid anatomy, should be carefully studied. If these innovations could be carried out, they might have the effect of giving a more scientific aspect to homœopathy, and of obviating some of the frequently repeated objections to it; but would they be of any practical utility to it? I believe not.

If greater precision were introduced into the diagnosis of natural diseases by homœopathists, and if a more complete consideration were given to other circumstances connected with them, such as morbid anatomy, etc., without a corresponding modification of, or addition to, the knowledge of medicinal diseases, it is evident the only possible result would be to lessen the similarity between the two classes of diseases. At present it must be admitted that a diagnosis of medicinal, corresponding to that of natural diseases, cannot be made; and that a knowledge of morbid structure can never be furnished by provings on healthy individuals. Besides, in connection with these modifications of Hahnemann's doctrine, we must not overlook the important fact that, so far as they have yet been carried out, they have not led to more successful treatment. It is generally allowed by the supporters of homœopathy of the present day, that the treatment of Hahnemann and his earlier coadjutors was at least as successful as their own.

I shall now give a summary of the more important conclusions at which I have arrived from an examination of the principle *similia similibus curantur*:—

1. The meaning of the term 'like' is so vague and indefinite, that in practice we find the principle *similia similibus* is very arbitrarily applied; in fact, its application does not admit of the precision and accuracy so necessary to therapeutics.

2. Medicines, when administered to healthy individuals, do not produce diseases similar to those which they can cure.

3. The insufficiency of the principle *similia similibus* as a therapeutic guide, has been admitted by many eminent homœopathic writers, and even by Hahnemann himself, in his work on chronic diseases.

CHAPTER VII.

HOMŒOPATHIC DILUTIONS—THE QUANTITY OF DRUG CONTAINED IN THEM CANNOT HAVE ANY MEDICINAL ACTION—THE ILLUSTRATIONS OF THE ACTION OF INFINITESIMAL DOSES SHOWN TO BE FALLACIOUS.

I SHALL now give a short account of the manner in which Hahnemann desired the homœopathic triturations and dilutions to be made, and likewise some calculations to show the amount of medicine contained in them. To prepare the infinitesimal doses, his practice was to add one grain or one drop of the concentrated or mother-tincture of the drug to thirty-three grains of sugar of milk in an unglazed porcelain mortar, with a pestle of the same material. After being slightly mixed with a horn spatula, the mass was triturated in the mortar for six minutes, and then scraped from the sides of the pestle and mortar, and stirred about for four minutes; the trituration was again repeated for six minutes, and the scraping for four. The second portion of thirty-three grains of sugar was then added, and the process repeated, as with the first one. Lastly, the third portion of thirty-three grains of sugar was added, and the rubbing and scraping alternately twice repeated, so that the whole process lasted an hour. This formed the first trituration of the drug. Each successive potency or trituration was prepared in the same manner, by rubbing together a grain of the preceding one, and ninety-nine grains of sugar of milk. The triturations were seldom made higher than the third, as Hahnemann supposed at that degree of it, all drugs, even those which in a crude state, or lower trituration, were quite insoluble in strong or diluted alcohol, became soluble in that vehicle. Potencies higher than the third were generally prepared in the liquid way.

To prepare the different solutions, Hahnemann commenced by taking one or two drops of the concentrated tincture, and ninety-eight or ninety-nine drops of alcohol, and shook them together in a phial for a variable length of time.[1] This formed the first solution, of which he took a drop and ninety-nine of alcohol to form by shaking the second; and so on *ad infinitum*, taking one drop of the preceding solution and ninety-nine drops of alcohol, and shaking them together to form the succeeding one. Hahnemann carried his solutions as far as the 30th potency; and whatever might be the medicine employed, this became his favourite dose in all diseases, especially in the latter part of his life. We know, however, from good authority, although he has not said so in any part of his works, that he occasionally used much higher solutions, as the 60th, and even the 100th. The following table is used by homœopathists to show the amount of a grain or drop of any drug contained in the different potencies, which are distinguished by the numbers 1, 2, 3, etc.:—

1st attenuation,	1 ($\frac{1}{100}$).	16th attenuation,	16 ($\frac{1}{100}$ quint.).
2d ,,	2 ($\frac{1}{10000}$).	17th ,,	17 ($\frac{1}{10000}$ quint.).
3d ,,	I. ($\frac{1}{1000000}$, a million).	18th ,,	VI. (a sextillion).
4th ,,	4 ($\frac{1}{100}$ mil.).	19th ,,	19 ($\frac{1}{100}$ sextil.).
5th ,,	5 ($\frac{1}{10000}$ mil.).	20th ,,	20 ($\frac{1}{10000}$ sextil.).
6th ,,	II. (a billion).	21st ,,	VII. (a septillion).
7th ,,	7 ($\frac{1}{100}$ bil.).	22d ,,	22 ($\frac{1}{100}$ septil.).
8th ,,	8 ($\frac{1}{10000}$ bil.).	23d ,,	23 ($\frac{1}{10000}$ septil.).
9th ,,	III. (a trillion).	24th ,,	VIII. (an octillion).
10th ,,	10 ($\frac{1}{100}$ tril.).	25th ,,	25 ($\frac{1}{100}$ octil.).
11th ,,	11 ($\frac{1}{10000}$ tril.).	26th ,,	26 ($\frac{1}{10000}$ octil.).
12th ,,	IV. (a quadrillion).	27th ,,	IX. (a nonillion).
13th ,,	13 ($\frac{1}{100}$ quad.).	28th ,,	28 ($\frac{1}{100}$ nonil.).
14th ,,	14 ($\frac{1}{10000}$ quad.).	29th ,,	29 ($\frac{1}{10000}$ nonil.).
15th ,,	V. (a quintillion).	30th ,,	X. (a decillion).

When we first read an account of Hahnemann's method of preparing medicines for homœopathic use, we have but a vague idea of the small amount of drug contained in infinitesimal doses; and it is only by calculations and comparative measurements that we can pretend to form a rather more definite notion of it. I shall give some extracts from those contained in Sir J. Y. Simpson's work on *Homœopathy*, p. 283, which are the most striking and complete that I have seen, and of which the accuracy seems to be admitted by homœopathists themselves:—

[1] He gave them from two to forty shakes.

'The subjoined arithmetical formula shows first the result, when we divide a quintillion of grains (the number contained in the first line of the formula) by 240 (in the second line), the average number of grains of sugar, or of sugar of milk, found by experiment to be contained in a cubic inch. Secondly, the quotient (in the third line) shows the number of cubic inches contained in a quintillion of grains of sugar; and this number, when again divided by the number of cubic inches in a cubic mile (stated in fourth line), yields the number of cubic miles of sugar contained in a quintillion grains of it. Lastly, when we divide this number of cubic miles (as given in fifth line) by the number of cubic miles contained in the mass of the earth (given in sixth line), we obtain the number of masses of sugar equal to the dimensions of the earth required to make a quintillion of grains of sugar:—

$$\frac{1,000,000,000.000.000,000,000,000,000,000}{240} \quad \text{Number of grains in a cubic inch.}$$

$$= \frac{4,166,667,000,000,000,000,000,000,000}{254,358,061,056,000} \quad \text{Number of cubic inches in a cubic mile.}$$

$$= \frac{16,381.000,000,000}{263,900,000,000} \quad \text{Number of cubic miles in the mass of the earth,}$$

$$= 61 \text{ globes of the bulk of the earth.}$$

'A quintillion of grains is expressed arithmetically by a unit, followed by thirty ciphers in the first line. A quintillion divided by 240, the number of grains contained in a cubic inch of sugar, gives 4167 quadrillions of cubic inches of sugar contained in the quintillion of grains. These 4167 quadrillions of cubic inches, divided by the number of cubic inches in a cubic mile—viz. 254,358,061,056,000—gives 16⅔ billions of cubic inches as the size of a mass of sugar containing a quintillion of grains of sugar. This, again, divided by 263,900,000,000, the number of cubic miles in the globe of the earth, gives, as the result, 61 globes of sugar of the dimensions of the earth in a quintillion of grains of sugar. The quantity of sugar, then, required for the reduction of one single grain of gold, or oyster-shell, or sulphur, or any other homœopathic drug, to the 15th trituration, or potency, is a mass equal to sixty-one times the size of the earth. And further, a portion out of this enormous mass, equal to a small pill or globule, is, according to the tenets of Hahnemann, a proper dose of the drug employed.'

'But he came latterly to use, as his constant and standard potency for gold and for all other drugs, a higher potency—viz. the 30th. In his last work, he of course (observes Dr. Dudgeon) orders the decillionth, or 30th dilution, to be given in every case. To reduce, however, a grain of gold or any other drug, or supposed drug, to this decillionth, or 30th dilution, would require the single grain of the medicine to be commixed through a mass of sugar immensely greater than for the quintillionth or 15th dilution. The mass can be easily ascertained by a simple rule of proportion; for if a quintillion of grains, or 1,000,000,000,000,000,000,000,000,000,000, are equal to sixty-one masses of sugar of the size of the earth, then a decillion of grains, or 1,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000, will be equal to 61,000,000,000,000,000,000,000,000,000,000 globes of sugar, each of the dimensions of our planet. In other words, the proportion of any drug in the 30th dilution, or the decillionth globules of the homœopathists, is to the sugar contained in the globules, as one grain is to sixty-one quintillions or spheres of sugar, each of these 61,000,000,000,000,000,000,000,000,000,000 being of the dimensions of the earth.

'The preceding calculations are founded on the supposition that the sugar of milk is the medicine employed to make the different potencies or dilutions: one grain of the drug, rubbed together for an hour with ninety-nine grains of sugar, forming the first trituration; and each successive trituration prepared in the same way, by taking one grain of the previous trituration, and rubbing it together with ninety-nine grains of sugar of milk. But generally, alcohol, or alcohol with water, is employed to prepare the attenuations or dilutions: with fluid substances it is employed as the medium of attenuation or dilution from the first, and with solid substances after the third trituration.

'The following table is constructed on the form of the preceding common table of homœopathic attenuations, and is drawn up for the purpose of showing the quantity of alcohol or fluid required to dilute or reduce one single grain or drop of any medicine down to the principal attenuations or potencies, as 1st, 5th, 15th, etc. The computations are made on the idea that 60 drops of fluid are equal, at least, to a drachm; 480

drops equal to an ounce; 160 ounces to a gallon; one gallon to 277 cubic inches; and 254,000,000,000,000 cubic inches to a cubic mile.

No. of the Attenuation, Dilution, or Potency.	Relative Quantity of Drug and Quantity of Alcohol in which it is dissolved in each Attenuation.
1st attenuation, ...	One grain or drop in one and a half teaspoonfuls of alcohol.
2d „ ...	One grain or drop in twenty-one fluid ounces of alcohol.
3d „ ...	One grain or drop in two thousand and eighty ounces of alcohol.
9th „ ...	One drop in a lake fifty fathoms in depth, and presenting two hundred and fifty square miles of surface.
15th „ ...	One drop in an ocean about forty-six thousand times greater than the whole waters contained in all the oceans of the earth.
30th „ ...	One drop in an ocean of fourteen septillions of cubic miles of alcohol, or 14,000,000, 000, 000, 000, 000, 000,000, 000,000, 000, 000,000,000 cubic miles.'

'The highest dilutions to which these calculations refer is the 30th, but many homœopathic practitioners frequently employ the 100th and 200th potencies; some employ more or less frequently the 400th, 600th, 800th, 1000th, 2000th, 10,000th, and even much higher. Dr. Nuñez of Madrid considers that the most appropriate dose for acute diseases is to be sought for in the 2000th dilution, and upwards; and he says chronic organic diseases are always aggravated by the 2000th potency, and must therefore be treated with much higher dilutions. To express the proportion of a grain or drop of medicine contained in the 100th potency requires a unit followed by two hundred ciphers, thus—1,000,000,000,000,000,000,000,000,000, 000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000, 000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000, 000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000, 000,000,000,000,000,000,000,000,000,000,000,000,000.' To represent the 200th potency would require a unit followed by four hundred ciphers, or double the number of those used above; and to represent the 2000th, a unit with several pages full of ciphers.

When we speak of the 1st, 3d, or 6th dilution of a drug, we have a vague idea that it contains an extremely minute quantity of it; but when we look at the long lines of ciphers which indicate the proportion of a grain or drop of medicine contained, for example, in the 30th dilution, and attempt to realize it in the 'mind's eye,' we find that the effort is quite useless. I shall afterwards show that the dynamization hypothesis is an absurdity; and no intelligent homœopathist will assert that mere mechanical division of the particles of a drug, however much it may facilitate their absorption, can give them millions of millions of times more power than they possessed in their natural state. To ask us, then, to believe in the medicinal power of the decillionth part of a grain of medicine, which in doses of a drachm may produce sufficiently marked but not severe effects, and in doses of a grain no perceptible effect whatever, would be to expect us to believe in what seems so contrary to the known laws of matter, that I am persuaded few, if any, even of the supporters of homœopathy, were they to consider the subject apart from clinical results, could possibly do so. I can well imagine that certain energetic remedies may act more or less in doses of the 1st, 2d, or 3d dilutions of the decimal scale;[1] but when it is a question of the medicinal power of a dose of the 15th, 30th, 100th, or 1000th potency —in which, as I shall afterwards show (at least in the case of metallic or insoluble drugs), there cannot be a particle of drug —scepticism becomes paramount.[2]

[1] In the decimal scale, one part of drug is mixed with nine parts of vehicle; in the centesimal, as we have seen, one part of drug is mixed with 99. Many homœopathists now use the decimal scale.

[2] There can be no doubt whatever that the most minute quantities of some drugs can produce sensible effects. Dr. Harley says, at page 223 of his work on the *Old Vegetable Neurotics:* 'I have myself once or twice experienced slight congestion of the entire conjunctiva, with dryness of the membrane, and dull, aching pain in the eyeball, lasting for several hours, after the use of a very weak solution of atropia. On one occasion this condition followed the instillation of twelve drops of a solution of one part of sulphate of atropia in 400,000 parts of water. It was accompanied by a dilatation of the pupil from one-ninth to one-seventh.'

If the medicinal power of a drug (calcarea, for example) be increased, however little, by each attenuation, it is evident that in doses of the 30th, 100th, 800th, or 2000th dilution, its action should become so manifest as not to admit of doubt; but this is so far from being the case, that a great many homœopathic practitioners refuse to believe in the medicinal power of the high dilutions.[1] Unfortunately, in the present state of the Materia Medica it is quite impossible to give a satisfactory answer to the important question—At what degree of dilution do the various medicines cease to produce any perceptible effect on the organism?

It is easy, however, to account for the belief of homœopathists in the efficacy of infinitesimal doses. Like many other practitioners, they attribute the recoveries which take place under their treatment, and which are brought about according to fixed laws by the natural resources of the organism, to the action of the drugs employed—the old besetting fallacy of medical observation, '*post hoc, propter hoc!*' But I would ask those supporters of homœopathy who have no faith except in the low dilutions, what cured those cases of disease in which the high dilutions which they think totally inert were employed? If the resources of the organism could accomplish the cure of these cases, why should they not be able to do the same in theirs?

It is a law of the material world to which there is no exception, that in proportion as we diminish the quantity of any substance (*other circumstances being the same*), we diminish at the same time the amount of its action. Homœopathists seem tacitly to admit this, by turning away so readily from calculations regarding the quantity of drug contained in their infinitesimal doses, to the results of their experience (the value of which I shall afterwards endeavour to estimate), and to the

[1] It is remarkable enough that the homœopathic practitioners who use the lower dilutions, make the same objection to the use of the high potencies that the opponents of homœopathy make to the infinitesimal doses in general, viz.: 'They contain such an infinitely small quantity of the drug, that they cannot possibly produce any effect.' The high dilutionists answer the objection of their brethren in the same manner as these do their opponents, by appealing to experience, and citing numerous cures.

phenomena exhibited by some external agents, whose action they consider analogous to that of their dilutions or potencies.

'We know,' it has been said, 'that a very small quantity of musk will continue to give out its peculiar odour for years without much loss of weight.' This fact only shows, what no one pretends to deny, that extremely minute quantities of matter are capable of producing sensible effects; but no one will say it is a case analogous to that of the action of homœopathic potencies. That extremely minute quantities of some substances can produce sensible effects, is one proposition; that by reducing largely the quantity of a substance—musk, for example—to the decillionth of a grain, its odour will not only not be diminished, but even greatly increased, is a very different one. It is the one, however, which must be affirmed by those who find an analogy between the action of musk and that of the dynamization of medicine, according to Hahnemann's hypothesis.

It has been asked, 'Can we measure the amount of effluvium left by a hare in passing through a field, that enables the dog to follow on her track?' Certainly not. If, however, the dog were to pass along the track a few days afterwards, would he recognise the odour of the hare? Certainly not. During that time, the effluvium emitted by the hare would be so much diminished in any given space, by mixing with the circumambient air, as to constitute perhaps a quintillionth of the original quantity,—a quantity too small to affect the olfactory nerves of the dog.

The action of minute doses of medicine is thought to be well illustrated by the powerful effects produced by various miasmata, which are supposed to act in very small quantities. Miasms are certainly very subtle agents, since they do not directly affect any of our senses, unless the peculiar odour occasionally found associated with their presence, can be ascribed to them. To suppose, however, that they act in very small quantities, is to make an unwarranted assumption; on the contrary, I should rather suppose that in some cases at least, when they act, they are present in large quantities. At all events, the effects of some of them are often manifest over a large extent of country at a distance from the place where they were formed. It has often been remarked that the crews of vessels sailing along, or at anchor near a pestilential coast, have been affected by emanations carried out from the land, where they were formed; whereas the crews of vessels sailing at the same time along the coast, but at a greater distance from it, have entirely escaped. But however that may be, no one can doubt that the more a miasm is diffused in the surrounding air, the feebler will its action become, till at length it ceases completely.

The interesting experiments of Spallanzani on the impregnation of the ova of the frog are frequently referred to by homœopathists, as affording a satisfactory illustration of the action of infinitesimal doses. His experiments have been partially repeated, with some modifications, by Dr. W. Arnold. Spallanzani found that water containing the 42,240th part of a grain of frog's semen, and Dr. Arnold that water containing only the 1,000,000th part of a grain, was sufficient to impregnate some of the ova exposed to its influence. At the present day it is generally admitted by physiologists, that in order to produce impregnation, a spermatazoon of the male must be united with an ovum of the female. Instead of speaking of a solution of semen, it would be more correct, perhaps, to say that the spermatozoa were suspended in and diffused through the water. In one important respect these experiments have no analogy with the minute subdivision of the infinitesimal doses of drugs; and it is this—the effective corpuscles concerned in impregnation were not dissolved or subdivided in the water, but diffused entire through its mass. Those drops only of the water which happened to contain one or more spermatozoa could cause impregnation.

Dr. Mayerhofer's microscopic examinations of various homœopathic attenuations of metallic or insoluble drugs are generally supposed by homœopathists to give great support to the opinion that the infinitesimal doses possess medicinal power, since the presence of particles of different metals have been detected in attenuations as high as the 11th and 12th. Admitting Dr. Mayerhofer's observations to be correctly made, I think it will not be difficult to show that they rather tend to throw doubt on the possible efficacy of the higher dilutions, without proving anything in favour of that of the lower ones. In the first place, homœopathists assume, what requires to be proved, that the minute particles of metals seen in the different triturations can produce sensible medicinal action; and in the second place, it is admitted that in the higher dilutions, whose efficacy is established on the same evidence as that of the lower ones—clinical experience—not a particle of drug can be detected, even with the aid of a powerful microscope.

A very simple calculation seems to prove that the higher dilutions cannot contain any particles of the metallic drug, and consequently that they cannot possibly possess any medicinal power. The diminution of the amount of drug at each trituration or dilution is a hundredfold; and if we assume that it is insoluble, and that we can estimate the number of particles contained in a given quantity of it, it will be easy to calculate at what attenuation it will disappear from the vehicle. Metallic gold, even when reduced to the state of a fine powder, is insoluble in alcohol; it is probably equally insoluble in the fluids of the stomach; and it cannot be absorbed from it or from the intestines in the state of particles, such as we see with the microscope in different triturations and dilutions.

Mayerhofer estimates the number of particles in a grain of the third trituration of arsenic or tin at 115,200,000; and as gold is reduced with more difficulty than these drugs to a state of fine comminution, we may estimate the particles of it in a grain of the third trituration at the same number. But to get a more convenient sum for calculation, let us nearly double the estimated amount, and fix it at 200,000,000.

If a grain of this trituration of gold be mixed with ninety-nine drops of diluted alcohol, a drop of the 4th dilution will contain only 2,000,000 particles of gold. If the dilutions be continued in the same manner up to the 7th, it is evident this last will contain only two particles of it. In making this calculation, it is assumed that the particles of gold are equally distributed amongst the ninety-nine grains of sugar of milk, or the ninety-nine drops of diluted alcohol; but on examination with the microscope, we find that this is not the case. The possibility of the absorption of solid particles from the intestinal canal into the vascular system, is by no means established. I incline to the opinion that every solid body must be dissolved before it can be so absorbed; and I think it is very doubtful, as

I have already said, if the fluids contained in the stomach or intestines can dissolve any amount of metallic gold.

The observations of Dr. Mayerhofer, then, give but little or no support to the supposed efficacy of infinitesimal doses. They can only be applicable to a small number of drugs, and to a limited number only of the attenuations even of that small number.

In an essay, *On the Great and on the Small in Nature,* published in the year 1837, by Professor Doppler of Prague, homœopathists fancy they have found a satisfactory physical explanation of the efficacy of small doses. 'Distinguishing that physical superficies of a body which is the sum of the surfaces of its constituent particles, from the sensible surface, which is the sum of the exposed surfaces of its exposed particles, he shows that the triturations practised by the homœopathic pharmaceutist increase the latter surface—the whole exposed surface that may be brought into reaction with the tissues—at a very rapid rate. A cubic inch of brimstone broken into a million of equal pieces, each of the size of a grain of sand, is magnified in sensible surface, from six square inches to more than six square miles. It is calculable in this way, that if each trituration of the homœopathist diminishes his drug a hundred times, the sensible surface of a single inch of sulphur, or any other drug, shall be two square miles at the third trituration; the size of all Austria, at the fifth; of Asia and Africa together, at the sixth.'[1] Professor Doppler concludes thus: 'We have said sufficient to show, that if medicines act in virtue of their mass, the doses used in homœopathy must be quite inert; but if in proportion to their surface, they may be of tremendous potency.'[2]

In this case, the unwarranted assumption must be made, that medicines act in proportion to their surface, which renders Doppler's calculations worthless; but even if that point were granted, they could only be applied to insoluble substances.

The last illustration which I shall notice, is one advanced by Mr. Hayes in the twelfth vol. of the *British Journal of Homœopathy,* p. 139. It is plausible, and it is the only one which has some analogy with the infinitesimal dilutions. The

[1] *British Journal of Homœopathy,* vol. i. p. 220.

[2] Black on *Homœopathy,* p. 88.

analogy consists in this, that while reducing the amount of vaccine matter in the same proportion as is done in preparing homœopathic medicines according to the centesimal scale, its power, if not increased, is certainly not diminished by any amount of successive dilutions.

To develope more fully his idea, the ingenious author adds some calculations—correctly made, I believe—which look as formidable as some of those which have been made against the infinitesimal doses. 'A strong man, sixteen stone in weight, can be inoculated with the hundredth part of a grain of vaccine matter; that is, he can be protected, in the vast majority of cases, from an attack of small-pox, by a dose of vaccine 107,520,000th part of his own weight. If from this man the 100th part of a grain of matter be taken, a second man of like weight with the first may be likewise protected from small-pox; that is, the second man is protected by the 10,000th part of a grain of the original vaccine, that is, by the 10,752,000,000th part of his weight. If we so proceed to inoculate ten men in succession, the tenth man is protected by a dose of the original vaccine 107,520,000,000,000,000,000,000,000th part of his own weight, or by 100,000,000,000,000,000,000th part of a grain of vaccine.'[1]

The author has overlooked one circumstance, however, which destroys the supposed analogy between the action of vaccine matter and infinitesimal doses, and so invalidates all his reasoning. He makes the unwarranted supposition, that in each successive vaccination a portion of the original 100th part of a grain of vaccine matter of the first vaccination is employed. I believe the whole of the matter inoculated is soon absorbed; and as that used for each subsequent vaccination is taken in every case from a newly produced secretion, I think it is very doubtful if what is taken for the second and subsequent vaccinations contains any particles of the original vaccine matter. In the present state of our pathological knowledge, it is impossible to give a decided opinon on this point. To suppose that corpuscles of the original vaccine matter are contained in that used for the second and subsequent vaccinations, is simply to beg the question.

[1] It is unnecessary here to say anything of the fermentation theory.

It is evident, then, that the facts brought forward by homœopathists, to which they attach great importance, and to which they frequently triumphantly refer, in order to illustrate the supposed action of infinitesimal doses, are not analogous to it, nor do they afford any support to that part of the homœopathic doctrine. Even the spectrum analysis, which reveals the presence of metallic matter in some pretty high homœopathic dilutions, says nothing in favour of their medicinal action on the body. It merely shows, like the other illustrations, that very minute quantities of matter sometimes produce sensible effects; but its revelations are quite opposed to the opinion that, in proportion as we subdivide a medicine, its power becomes much greater, or at least relatively greater. On the contrary, they confirm the universal law, that by diminishing sufficiently the quantity of any substance, we diminish at the same time (other circumstances being the same) the amount of its action. To suppose that some metallic 'dilutions,' in which the presence of inconceivably minute quantities of metal have been detected by means of very delicate tests, can produce any perceptible medicinal action on the body, is to make a completely unwarranted supposition.

In conclusion, then, it must be said the supposed medicinal power of infinitesimal doses is so completely opposed to the universal law of nature, that, however explained, it cannot be entertained for a moment.

CHAPTER VIII.

SECTION I.—THE ORIGIN OF INFINITESIMAL DOSES—INCREASED SENSIBILITY OF THE DISEASED ORGANISM TO THE ACTION OF DRUGS—THE HOMŒOPATHIC AGGRAVATION OF DISEASE SHOWN TO BE UNFOUNDED.

I HAVE frequently seen it stated in works written by homœopathists, that after Hahnemann had discovered the law *similia similibus*, he gradually diminished the doses of the drugs which he employed, until he adopted the use of infinitesimal ones, in consequence of having often remarked that severe, and even dangerous, aggravations of diseases were produced by the ordinary doses. I believe this statement to be entirely erroneous. In two papers which he published in the year 1797, the one entitled *Some kinds of Continued and Remittent Fevers*, the other *On some Periodical and Hebdomadal Diseases*, we find that he prescribed the ordinary allopathic doses of medicine; and he makes no remark that would lead us to suppose that he saw any necessity for diminishing them. In fact, some of those which he prescribed at that time were very large. For example, he sometimes gave 30 to 40 grains of camphor in twenty-four hours. At page 37 of the second vol. of his translation of the *Edinburgh Dispensatory*, published in 1798, he says: 'The most active medicines, even in considerable quantities, produce only good, no bad effects.'

From the pamphlet which he published in the year 1801, *On the Cure and Prevention of Scarlet Fever*, it appears that he began to use the infinitesimal doses during the latter half of the year 1799. He used the $\frac{1}{5000000}$th part of a grain of opium, and the $\frac{1}{2000}$th part of a grain of ipecacuanha, administered well mixed with one to four table-spoonfuls of water, and, as he informs us, with satisfactory results. 'I could not imagine,' he says, 'a more suitable mode of treatment so rapid and certain in its results.' It is remarkable enough, however, that he does not attempt to give any explanation, or assign any reason for this sudden and enormous diminution of the doses which he previously employed. He says nothing about the injurious effects of ordinary or allopathic ones; he only remarks in a note, 'that the smallness of the quantity in which the medicine that acts on the whole system of the living organism, when it is suitable to the case, produces its desired effect, is incredible.' Whatever the reason was, we may safely conclude it was neither the result of observation nor of experiment.[1]

In the next paper, *On the power of small Doses of Medicine,* which he published in the same year, 1801, his ideas on the subject are more developed. In reply to Hufeland's query, 'What effect can the $\frac{1}{100000}$th part of a grain of belladonna have?' he remarks: 'A very hard dry pill of extract of belladonna produces in a robust, perfectly healthy labourer usually no effect. But from this it by no means follows, that a grain of this extract would be a proper or too weak a dose for this or a similar stout man if he was ill, or if the grain were given in solution. The most healthy, robust thresher will be affected with the most violent and dangerous symptoms from one grain of extract of belladonna, if this grain be dissolved in much (*i.e.* two pounds) of water by rubbing, the mixture (a little alcohol being added, for all vegetable solutions are rapidly decomposed) made very intimate by shaking the fluid in a bottle for five minutes; and if he be made to take it by spoonfuls, within six or eight hours.' 'These two pounds of water will contain about 10,000 drops. Now, if one of these drops be mixed with other 2000 drops (8 oz.) of water mixed with a little alcohol, by being vigorously shaken, one teaspoonful (about 20 drops) of the mixture given him every two hours will produce not much

[1] The resolution to reduce so enormously the doses of drugs was evidently rather suddenly formed. In Germany, medical men were not allowed to prepare and dispense the medicines required by their patients. As Hahnemann attempted to do so, he made himself obnoxious to the apothecaries; and it was probably to enable himself to defeat their opposition, that he adopted the use of small doses whilst he was residing at Königslutter.

less violent symptoms in a strong man, if he is ill.' 'A few teaspoonfuls of this mixture will bring him to the brink of the grave if he was previously regularly ill, and if his disease was of such a description as belladonna is suitable for.'[1] 'The hard pill slides almost completely undissolved over the surface of the intestinal canal. Very different is it with a solution, particularly with a thorough solution. Let this be as weak as it may, in its passage through the stomach it comes in contact with many more points of the living fibre; and as the medicine does not act atomically, but only *dynamically*, it excites much more severe symptoms than the compact pill containing a million times more medicine.'

Further on he says: 'To the ordinary practitioner, it is incredible that a given person, when sick, needs only to take the millionth part of the same drug that he swallowed when well, without its having any particular effect, in order to be violently acted on; and yet this is undeniably the case. It is a fact, that in disease the preservative power, together with the subordinate nameless forces (some of them almost resemble the instinct of animals), is much more excitable than in health.' In this essay he indicates the necessity for giving the carefully diluted medicine in small doses, on account of the immense power which it gains by intimate mixture, and on account of the greatly increased susceptibility of the diseased organism; and lastly, he remarks, for the first time, that medicines act *dynamically*, not *atomically*,—an opinion which may be considered as the original idea of his celebrated theory of dynamization.

I may here advert to one of the numerous contradictions which occur in Hahnemann's works. 'Will medical men ever learn how small, how infinitely small, the doses of medicine may be, in order to affect the system powerfully, when it is in a morbid state? Yes! they affect it powerfully when they are chosen improperly: new, violent symptoms are added; and it is usual to say (whether correctly or not, this is not the place to decide) the disease has undergone an aggravation.' About half a page further on, he makes a statement quite contradictory of what he has just affirmed: 'How highly important, on the

[1] Hahnemann's *Lesser Writings*, p. 444.

other hand, is it, that in the event of the remedy being improperly selected, such a small dose can seldom excite such serious symptoms, ordinarily termed aggravations of the disease!'

In his essay entitled *The Medicine of Experience*, published in the year 1805 (one of the best which he has written), he developes still more his notions respecting the sensibility of the organism in disease: 'None but the careful observer can have any idea of the height to which the sensitiveness of the body to medicinal irritation is increased in a state of disease. It exceeds all belief when it has attained a great intensity.' In speaking of the dynamic action of medicine, he says: 'Like the vitality itself, by means of which it is reflected upon the organism, it is almost purely spiritual in its nature. The only condition necessary for the full action of the properly chosen homœopathic medicine is, that a portion of it (it is of little, almost of no, importance how small the dose is) should come in contact with the sensitive living fibre.' 'This dynamic property is so pervading, that it is quite immaterial what sensitive part of the body is touched by the medicine, in order to develope its whole action, provided the part be but destitute of the coarser epidermis—immaterial whether the dissolved medicine enter the stomach, or merely remain in the mouth; be applied to a part deprived of skin, introduced into the rectum, or applied to the lining membrane of the nostrils.'

Before proceeding further, it will be necessary to advert a little to the increased sensibility of the diseased organism to the action of medicines,—a point which is much insisted upon by homœopathists even of the present day. Hahnemann undoubtedly exaggerated its amount, especially about the time when he wrote his essay, *The Medicine of Experience*; and I cannot help thinking that on this, as on many other points of homœopathy, his opinions were very much influenced by his theoretical views at the time. The reason why he attached so much importance to the increased sensibility of the organism in disease was, that it enabled him to give a more satisfactory explanation of the action of infinitesimal doses; at all events we know that, after he had completed his theory of dynamization, which rendered the extreme sensibility of the diseased organism a less important element in the elucidation of the action of minute doses, he did not seem to attach much importance to it.

It cannot be doubted that, in many cases of disease, especially when they are of an inflammatory nature, the sensibility of the affected part or organ to those medicines which act on it is decidedly increased. Thus, in a case of acute inflammatory diarrhœa with gripes, an ordinary dose of an irritant purgative would, in the majority of cases, increase the severity of the symptoms; and in an inflammatory state of the kidneys or bladder, the ordinary doses of cantharides would aggravate the complaint. In cases of tetanus, the usual doses of strychnine increase much the severity of the spasms. Although we cannot indicate with anything like precision the amount of increased sensibility, we may reasonably conclude, from the visible effects of the local application of homœopathic remedies in allopathic doses to inflamed parts, that it is not nearly so great as Hahnemann would have us believe. Thus, in a case of burn that occurred in his own practice, and which he cited in order to illustrate the action of the homœopathic law, *similia similibus*, the constant application of warm alcohol to the affected part was very beneficial; and the good effects which follow the use of stimulating collyria to the inflamed mucous membrane of the eye, and of stimulating gargles to that of the throat, particularly when the more acute stage has passed, are well known. Besides, there are many morbid states in which the sensibility of the organism (even to homœopathic medicine), far from being above the natural amount, is evidently abnormally diminished. I have seen cases of constitutional syphilis, in which unusually large quantities of mercury required to be given to cause perceptible action on the gums; and in *delirium tremens* it frequently happens that the ordinary dose of opium must be much increased, in order to produce a narcotic effect.

I would here call attention to a circumstance connected with the increased sensibility of diseased parts to the action of medicine, which seems to have been overlooked by homœopathists. If the injurious effects of an ordinary dose of medicine be in proportion to the increased sensibility of the system generally, or of any diseased organ in particular, it is evident that they may be avoided, by diminishing the dose in proportion to the increase of the sensibility. If, for example, in any given disease, the sensibility to the action of a homœopathic medicine be five, ten, or twenty times greater than in the healthy state, one grain of the first trituration of a medicine (of which one grain in the undiluted state produces only moderate action on a healthy individual) would be a diminution of the dose more than enough to obviate any injurious effects from it. As I have already remarked, we are not able to estimate even approximatively the increased amount of sensibility in disease. Some suppose it to be, at most, three or five times greater than in the natural state. In no case, however, will any one think it 100, 1000, or 1,000,000 times greater; consequently, in a grain of the 2d or 3d dilution, the quantity of the drug would be reduced so much as to be out of all proportion to any increase of sensibility that could be reasonably imagined. In this respect, the 30th, 100th, 200th, 20,000th, etc. dilutions are quite preposterous.

From what has been said, I think it will be admitted that Hahnemann greatly exaggerated the amount of sensibility of diseased organs to medicinal irritation; since we have seen that, even under circumstances where theoretically the most injurious effects might have been expected from the external, and also from the internal, use of large quantities of homœopathic remedies, the most beneficial results, on the contrary, were obtained. However, I consider the modification of the sensibility of diseased organs to the action of medicine an extremely interesting subject for examination, and one which has not received anything like the attention which its importance demands from allopathic practitioners.

Hahnemann speaks in his essay, *The Medicine of Experience*, for the first time of the aggravation (to which he scarcely alluded in his last paper) caused by too large doses of homœopathic medicine, or by the use of improperly chosen drugs. He says: 'If we have not only selected the right remedy, but have also hit upon the proper dose (and for a curative purpose incredibly small doses suffice), the remedy produces, within the first few hours after the first dose has been taken, a kind of slight aggravation (this seldom lasts so long as three hours), which the patient imagines to be an increase of his disease, but which is nothing more than the primary symptoms of the medicine, which are somewhat superior in intensity to the disease, and which ought to resemble the original malady so closely, as to deceive the patient himself in the first hour, until the recovery that ensues after a few hours teaches him his mistake.' This he called homœopathic aggravation. 'If, however, the first dose of the perfectly adapted curative medicine was not somewhat superior to the disease, and if that peculiar aggravation did not occur in the first hour, the disease is, notwithstanding, in a great measure extinguished, and it only requires a few, and always smaller, doses to annihilate it completely.'

'But the case is quite different with palliative treatment, when a medicine is employed whose positive primary action is the opposite of the disease. Almost immediately after the administration of such a medicine, there occurs a kind of alleviation, an almost instantaneous suppression of the morbid irritation for a short time, as in the case cited above of the cold water applied to the burned skin. Such a drug is called a palliative.'

'Palliatives prevent the impression of the morbid irritation on the organism only as long as their primary symptoms last, because they present to the body an irritation that is the reverse of the irritation of the disease. Thereafter their secondary action commences; and as it is the opposite of their primary action, it coincides with the original morbid irritation, and aggravates it. As in the positive curative mode of treatment in the first hour a slight aggravation usually ensues, followed by an amelioration and recovery all the more durable; so in the palliative method there occurs in the first hour, indeed almost instantaneously, a deceptive amelioration, which diminishes, however, from hour to hour, until the period of the primary, and in this case palliative, action expires, and not only allows the disease to reappear, as it was before the use of the remedy, but somewhat of the secondary action of the medicine is added, which, because the primary action of the remedy was the opposite of the disease, now becomes the very reverse—that is to say, a state analogous to the disease. This state is an increase, an aggravation, of the disease.'

In the last edition of the *Organon*, Hahnemann expresses similar notions with regard to medicinal aggravation: 'This slight homœopathic aggravation, during the first hours, is quite as it ought to be, as the medicinal disease must naturally be somewhat stronger than the malady to be cured, if it is to overpower and extinguish the latter.'

If we admit Hahnemann's opinion to be correct, that the medicinal disease must be slightly stronger than the natural one in order to cure the latter, a certain increase of the severity of the symptoms—or, in other words, a certain amount of aggravation—is inevitable; it must occur if the theory be true. Since the time when Hahnemann published his views respecting medicinal aggravation, great diversity of opinion has existed on this point amongst his followers: some denying its existence altogether, others admitting its occasional production by large doses of homœopathic medicine: some, particularly those who use habitually high potencies, finding it of frequent occurrence; and others occasionally remarking it during the use of every variety of dose, from the lowest to the highest. It certainly has been most frequently spoken of by those who habitually employed the high or very highest potencies, or, in other words, the smallest doses—infinitely smaller than those which Hahnemann used, and where, consequently, the aggravation could not be ascribed to too large doses. From the general evidence of homœopathic practitioners, especially of those who have chiefly used the larger doses, or lower potencies, we have reason to conclude that aggravation of disease from too large doses of homœopathic medicine is of rare occurrence.

It is remarkable enough, that Hahnemann does not cite a single satisfactory case of homœopathic aggravation. That of colicodynia, so often referred to, which was dangerously aggravated by an overdose of a powerful medicine, veratrum album, of which 16 grains were taken in two days (a dose sufficiently large to cause severe symptoms if administered even to a healthy person), cannot be placed in the category of cases in which the supposed aggravation has been caused by the use of the inconceivably minute homœopathic doses.

If his opinion be true, how is it that, in the numerous cures cited in the *Organon*, in which large doses of medicines homœopathic to the diseases had been used, we hear nothing of their aggravation? How is it that Hahnemann himself, in the numerous cures both of acute and chronic diseases which he made previously to the year 1800, with allopathic doses of medicines, remarked no aggravation? How is it, to speak of two diseases only, that intermittent fever is daily and often rapidly cured with large doses of quinine, and gout with large doses of colchicum, without any aggravation being noticed? Hahnemann's doctrine of homœopathic aggravation is untenable. Besides, how could one distinguish the supposed effects of medicines from a natural aggravation or exacerbation of the disease which so frequently takes place in acute cases?

SECTION II.—WHEN HOMŒOPATHIC GLOBULES WERE FIRST EMPLOYED—AN ACCOUNT OF THE DYNAMIZATION HYPOTHESIS—IT IS SHOWN TO BE UNFOUNDED.

In the first edition of the *Organon,* published in the year 1810, Hahnemann reproduces the opinion which he had already advanced respecting the small doses of medicine necessary to overcome disease, and also the one that, by dividing a dose, so as to take it diluted at several times, a much greater effect will be produced, than if it had been taken at once. This increase of power in divided doses he ascribes to the greater extension of the medicine through the liquid with which it has been intimately mixed; and in order, therefore, to make the homœopathic dose sufficiently small, it should be given in the smallest possible volume, so as to touch as few nerves as possible. Although he formerly gave the medicine in water, he now considered it injurious to do so, or even to drink water after taking it; as the mere dilution, without succussion, would increase its activity,—an opinion which he formerly rejected. Subsequently, however, he gave up that notion, and resumed the practice of giving the medicine dissolved in water, and in the last years of his life even in large quantities of it. In the same work, he considers it impossible, on account of the various strengths of the medicines themselves, to fix on any dose that could be considered always proper.

His professed object in diluting medicine was still to avoid aggravation of the disease; and from what he says of the smallest doses still containing a certain amount of the drug, which acts dynamically, it is evident that his opinions respecting the dilution of medicine had not undergone any particular change for several years. From the year 1810 till the year 1827, he seems not to have had any fixed standard for the doses he employed, which varied from the mother-tincture up to the 30th dilution, sometimes higher, sometimes lower; but during that period there was a gradual tendency to employ more and more frequently the higher potencies.

About the year 1812, Hahnemann probably began to use his famous globules of milk-sugar as vehicles for administering medicines. At first they were made of various sizes, the largest weighing $\frac{1}{30}$th, and the smallest $\frac{1}{300}$th part of a grain of sugar of milk. They were medicated by being touched with the moistened stopper of the phial containing the attenuation to be communicated to them. Two or three drops of the liquid were sufficient to moisten a thousand of them. Afterwards he employed the smallest, those of the size of a mustard seed, chiefly for olfaction, and those of the size of a poppy seed for taking internally. So potent did he consider the medicine which they contained, that (as he more than once affirmed), after having been kept from eighteen to twenty years, and repeatedly used for olfaction during that time, their medicinal virtue remained unimpaired,—an opinion which few, if any, of his disciples will accept at the present day.

In the year 1827, in an article in the last volume of his *Materia Medica Pura,* he gave an account of his theory of dynamization. He affirmed that, by means of succussion and trituration, 'the dynamic or medicinal powers of drugs became so remarkably developed, that they not only counterbalanced the diminished power naturally occasioned by their diminished quantity, but actually increased their power and energy to an almost infinite degree, till at last their material substance seemed to be transformed into pure medicinal spirit.' To show that simple solution has nothing to do with this wonderful development of the properties of medicines, and which can only be accounted for by the effects of succussion or trituration, he cites the following experiment: 'I dissolved a grain of soda in an ounce of water, mixed with alcohol, in a two-ounce phial, and shook the solution continuously for half an hour, by which the solution was rendered in dynamization and energy equal to the 30th development of potency.' Unfortunately this is not the only question in homœopathy that has been settled rather by the *ipse dixit* of Hahnemann than by the result of laborious comparative experiments.

'The homœopathic attenuations, however, so far from being a diminution of the medicinal power of a drop or grain of the crude drug, keeping pace with the extreme fractional diminution, as expressed by figures, that, on the contrary, experience shows them to be rather an actual exaltation of the medicinal power, a real spiritualization of the dynamic property—a true, astonishing, unveiling and vivifying of the medicinal spirit.'[1] 'By these processes, the internal medicinal power is liberated from its natural bonds, so as to enable it to operate more penetratingly and more freely on the living organism;' and, using almost the very words of Paracelsus, he adds: 'The material receptacle of these natural forces—the palpable, ponderable matter—is not to be taken into consideration.'

In his article on Thuja in the *Materia Medica*, he says, speaking of the 30th, and even of the 60th dilution, 'that if each dilution be shaken ten or more times, so far from being inferior in strength to the lower dilutions, it is actually more powerful; and we are warned against successing the different dilutions too much, in consequence of the dangerous effects which they may produce. For example, a drop of Drosera of the 15th or 30th dilution, each of which has had twenty shakes, will, from its extreme potency, endanger the life of a hooping-cough patient; whereas had each dilution only been shaken twice, a globule of the same dilution would cure the disease without endangering the child's health in the slightest degree.' It was in consequence of the effects supposed to be produced by too much succussion, that Hahnemann urged physicians not to carry about with them liquid medicines, as the mere shaking from the motion of walking or driving might potentize them too much.

At this period he advised each dilution of the drug to be prepared with two shakes only, on account of the danger that might

[1] Hahnemann's *Lesser Writings*, p. 823.

be caused by using preparations whose power had been too much increased by more succussion; but in the second edition of his work on *Chronic Diseases,* he advises ten succussions to be used in preparing each potency; and subsequently he says, one may give thirty, forty, fifty, and even more strong shakes, each made against some elastic body, and six or eight shakes more, each time a dose is taken.

It appears, then, that Hahnemann's opinions respecting the doses of medicines, and the manner of preparing and administering them, frequently underwent changes or modifications, which were sometimes so opposed to one another as to be quite contradictory; and as he seldom formally renounced previous opinions when he adopted new or even contradictory ones on any given point, it is often very difficult to ascertain exactly what were those which he actually held.

When speaking of the properties of drugs, Hahnemann does not attach a clear and precise meaning to the word power. In general, he seems to use it as synonymous with property; but in one or two places of his works he evidently considers power to be an entity—a *quid spirituale.* 'Medicinal substances,' he says, 'are not dead masses in the ordinary sense of the term; on the contrary, their true essential nature is only dynamically spiritual—is pure force;'[1] and again, 'till at last their material substance seems to be transformed into pure medicinal spirit.'[2]

In the introduction to this work, I have defined power to be an abstract term, expressive of the relation of cause and effect. The properties or qualities, or, as Hahnemann would have said, the powers of bodies, are merely terms expressive of certain changes or effects which they produce on ourselves or on surrounding bodies. They do not represent entities, or something apart from bodies themselves. We say snow is white, because by its action on the organ of vision it produces in our minds the sensation of whiteness; sugar, dissolved in the mouth, causes the sensation of sweetness, and we say sugar is sweet.

When a spark is applied to gunpowder, it produces an explosion; and we say that it is a property or quality of an ignited body, when applied to gunpowder, to make it ex-

[1] *Lesser Writings*, p. 822.

[2] Note on Thuja in the *Reine Arzeneimittellehre.*

plode; that the application of an ignited body to gunpowder is the cause of its explosion, or that an ignited body has the power to make gunpowder explode. To say, then, that a body has the power to produce a certain change or effect—that it is a property or quality of the body to do so, or that the body is the cause of the effect—is to express the same idea in different words: that one body, placed in a certain relation to another, will be invariably followed, *other circumstances being the same*, by the same change or effect. To talk, then, of bringing out or separating any property of matter from the substance that possesses it, and of transferring it to other matter, is simply absurd; and the idea of transmuting matter by mere friction into pure spirit, or even into any other kind of matter, is so contrary to all the known phenomena of the material world, that Hahnemann was bound to give some satisfactory proof of the truth of his opinion: he gave none, and could not give any.

The dynamization hypothesis, then, is not supported by the slightest proof, or rather it involves an impossibility; and the ablest homœopathists of later times have rejected it. However, a number of homœopathic practitioners—consisting chiefly, I believe, of those who employ the high dilutions—still believe in its soundness. It owed its existence apparently to the difficulty of giving anything like a reasonable explanation of the action of infinitesimal doses. The subject occupied Hahnemann's thoughts for a period of twenty-five or thirty years before it assumed the form in which it was given to the world. In the earlier period of homœopathy, the extreme subdivision of the drug, and the much exaggerated sensibility of diseased organs to the action of medicine, were considered sufficient to remove the difficulty. He held the dynamization hypothesis, without making any important modification of it, for the rest of his life.

SECTION III.—DYNAMIZATION CARRIED STILL FURTHER—IN HOMŒOPATHY THERE IS NO FIXED RULE FOR THE CHOICE OF A DOSE—THE SUPPOSED EFFECTS OF COMMINUTION OF DRUGS ARE CONTRADICTORY.

A few years after the first announcement of the dynamization hypothesis, Count Korsakoff published, in the year 1833, an article in the 11th and 12th vols. of the *Archiv. f. homœopath. Heilkunde*, in which he informs his readers that, by a legitimate application of Hahnemann's principles, he had carried the dilutions as high as the 1500th; and that the curative results obtained by the use of these high potencies were most satisfactory. He likewise gives an explanation of the transmission of the medicinal properties of a dry medicated globule to a large number of globules of sugar of milk by means of friction, without employing Hahnemann's process. Thus, if a globule medicated with the 1500th dilution be put into a phial containing 13,500 unmedicated globules (and sufficiently large to admit of free succussion), and well shaken for five minutes, all the 13,500 will acquire the same medicinal properties as the previously medicated one. He explains this wonderful phenomenon by supposing that the medicated globule infects the others, after the manner of an infectious disease; indeed, he supposes that in all dilutions above the 3d or 6th, the unmedicated vehicle receives medicinal qualities in this manner. Korsakoff's notions with regard to dilution, and the transmission of medicinal properties, were probably suggested by Hahnemann's statement, that 'a medicated globule continued to give out medicinal power during a period of twenty years, without losing any of its strength.'

Hahnemann seemed hesitatingly to admit the utility of Count Korsakoff's high dilutions; but he considered his infection hypothesis a very ingenious and probable one. In his remarks on Korsakoff's paper he says, that the extent to which dilution may be carried, without loss of medicinal strength, is quite illimitable. Probably fearing, however, the additional ridicule that would be thrown on homœopathy by these high dilutions, and the increased scepticism with which the new system would be received by medical men, he advised homœopathic practitioners not to go beyond the 30th dilution, for the following reasons: 'There must be some end of the thing; it cannot go on to infinity. By laying it down as a rule that all homœopathic remedies be diluted and dynamized up to 30°, we have a uniform mode of procedure in the treatment of all homœopathists; and when they describe a cure, we can repeat it, as they and we operate with the same tools.' The former part of the quotation contains no reason; and the remark which he makes in the latter part, about operating with the same tools, could be quite as correctly said of any other dilution as of the 30th. Hahnemann was evidently sadly embarrassed by the perverse zeal of some of his supporters, who seemed resolved on practically working out a *reductio ad absurdum* with his slowly developed dynamization hypothesis.

Hahnemann did not live to see to what an extreme length the celebrated Jenichen—starting probably from one of his former assertions, 'that by means of succussion continued uninterruptedly for half an hour, a grain of soda, dissolved in an ounce of diluted alcohol, acquired a power equal to that of the 30th'—would carry potentizing. From the 30th, Jenichen gradually ascended to the 100th, 200th, 400th, 1000th, 2000th, 10,000th, 20,000th, and even 40,000th dilution. In preparing his potencies, he deviated considerably from the plan proposed by Hahnemann; so that a given potency of Jenichen corresponds to a much lower one in the Hahnemannian scale. Stapf, Gross, Hering, Rummel, and others, were enchanted with the results obtained in practice from the use of Jenichen's high dilutions. Gross enthusiastically exclaims: 'Talk of your model cures!' (in allusion to some cases published by Hahnemann); 'they are nothing at all in comparison with the results obtained by the high potencies.' He and several other physicians published a large number of cases, in which most remarkable cures had been accomplished by means of them.

I should remark that many homœopathic practitioners have no faith whatever in the higher, or even in the high (6th–30th) potencies; and it is remarkable enough, that the principal reason urged against the use of them is the very one which the opponents of homœopathy urge against the infinitesimal doses in general—'the impossibility of such minute doses having any action.' The high dilutionists reply to the objection by an appeal to experience, and cite a large number of successfully treated cases to justify their practice. Their opponents have no well-established facts on which they can base their assertion, that a given potency cannot have any medicinal action, on account of the minuteness of its dose. They cannot pretend to say what amount of drug is necessary to enable it to produce medicinal action, or in other words, at what degree of attenuation it ceases to manifest it; consequently their objection applies with as much force against the 5th, 15th, or 30th, as against the 100th or 1000th dilution. In the meantime, the cures made by the high dilutionists are opposed to the assertion of their opponents, and no one as yet has been able to give any rule for guidance in the choice of a dose. *In this respect there is no pretension to any fixed rule in homœopathic practice:* each practitioner chooses one according to his individual opinions or experience.

The truth of this remark is confirmed by homœopathic writers. 'In fact, we may almost say there are as many opinions (in reference to the dose) as there are practitioners; and each is prepared to prove the superiority of his own by an imposing array of cases.'[1] 'We feel convinced that few, if any, have been led to the adoption of their favourite doses from patient and careful trial of all the various attenuations; and we are confident that all who have employed indifferently the medicines in all dilutions, would be greatly at a loss to determine which dilutions are the most efficacious, or decide which they could best dispense with in some classes of diseases.'[2] Homœopathic practitioners generally say that satisfactory cures may be accomplished with all potencies; but the greater number seem to prefer the lower ones in acute, and the higher ones in chronic diseases. I believe many powerful medicines, such as mercury or arsenic, can produce sensible medicinal effects, particularly on certain individuals, in much smaller doses than allopathic practitioners generally suppose.[3]

If it be true that the power of a medicine—charcoal, for example—is sensibly increased, however little, at each dilution, this increase at the 100th, 1000th, or 2000th dilution should be unmistakeably manifest to all observers; but from what has been said, it is evident that no such gradation of activity is observed on using these high potencies.

The objects which homœopathists profess to have in view in diluting medicines are two: 1*st*, To diminish the strength of the drug, and so prevent aggravation of the disease from its use;

[1] *Brit. Jour. of Hom.* vol. v. p. 257. [2] *Ibid.* p. 106.

[3] See note 2, page 87.

2*d*, To develope medicinal power in drugs which are inert, or nearly so, in their crude state, as calcarea, silex, etc.

In the introduction to the article on arsenic in his *Materia Medica Pura*, Hahnemann says: 'If the $\frac{1}{10}$th part of a grain of arsenic be in many cases a dangerous dose, must not a $\frac{1}{100}$th part be much milder ? And if this is the case, must not every further diminution of the dose be still milder ?' Let us suppose, then, several successive dilutions of a grain of arsenic made according to the formula, the result would be a gradual diminution of the quantity of the drug, and consequently of its effects, as Hahnemann has said. Let us likewise suppose a grain of calcarea prepared in the same manner, the result would be a gradual diminution of the quantity of the drug, as in the case of arsenic; but according to homœopathists, with a marked development of its medicinal power. But how can exactly the same mechanical process develope in the case of carbonate of lime supposed medicinal properties, and in the case of arsenic not exercise any perceptible influence whatever, except that of diminishing its power? The dynamization hypothesis could not account for the supposed effects produced by friction on the calcarea, as we showed when discussing that subject; besides, according to that hypothesis, friction ought to develope and increase the medicinal properties of the arsenic as well as those of the calcarea. In the same article Hahnemann has very justly said: 'Now, if arsenic, like every other powerful medicinal substance, can, by merely diminishing the dose, be most effectually rendered so mild as to be no longer dangerous to life, then the only thing which remains to be discovered by experience, is how far the dose must be diminished, that it shall be small enough to produce no evil consequences, and at the same time large enough to be efficacious as a remedial agent in those diseases for which it is adapted.'

As we formerly had occasion to remark, the majority of homœopathic practitioners have now abandoned the dynamization hypothesis; and the supposed development of medicinal properties (of calcarea, for example) by friction in a mortar, or by succussion in a phial, is generally accounted for by the extreme comminution of the particles of the medicine produced by these processes, which enables them to act more powerfully on the organism; but friction must produce the same physical effects on arsenic, of which, however, it is not supposed to increase the power. It follows, then, that the opinions respecting the effects produced by the comminution of drugs involve inconsistency and contradiction: they admit that the same cause, operating under similar circumstances, can develope and increase medicinal power in one substance; whilst in another, on the contrary, it diminishes it.

When a drug exists in a solid state, and is but slightly soluble, there can be no doubt that, by reducing it to a state of fine comminution, we facilitate its partial solution, and subsequent absorption into the system; but it seems very doubtful if any solid substance, however fine its particles may be, can be absorbed, as such, from the stomach or intestines into the vascular system. Numerous experiments have been performed by several physiologists in order to determine this point, but the results hitherto obtained by them are conflicting; and in the present state of pharmaceutical knowledge, no one can say how far trituration must be carried out, in order to develope completely the medicinal action of any drug. When it is soluble, or exists in the liquid state, I believe the active succussion of it with a proper amount of menstruum for half a minute will be quite enough to ensure a sufficiently minute subdivision of it for all practical purposes.

I shall now give a brief summary of the principal points that have been discussed in this chapter. We found that Hahnemann had greatly exaggerated the sensibility of the diseased organism to the action of homœopathic drugs, in order probably to be able to give some explanation of the sudden and enormous reduction of their doses which he had made some years previously,—a reduction totally unwarranted by any known observations or experiments: had he made any with reference to it, he would not have failed to have spoken of them.

Hahnemann thought that, in every case which was cured, the natural disease was overcome or removed by the stronger medicinal one; and with that theoretical notion, it is not surprising that he was constantly meeting with cases of homœopathic aggravation caused by the too great strength of the drug. Confused and conflicting views were held on this subject by his followers; but there can be no doubt, according to the most eminent of them, that homœopathic aggravation is of rare occurrence. As it is generally admitted to be much rarer from the use of the lower than of the higher dilutions, it is evident that it cannot be owing to the largeness of the dose, which is inconceivably smaller in the latter than in the former.

The infinitesimal subdivision of drugs, therefore, is a practice totally uncalled for, and which can only be regarded as a theoretical mistake, to say nothing of the contradictory results to which it leads. In the preceding chapter I have shown that they cannot possess any medicinal power in that state. The dynamization hypothesis was found to be quite untenable.

CHAPTER IX.

PROVINGS OR TRIALS OF DRUGS MADE BY HAHNEMANN AND HIS FOLLOWERS—DEFECTS AND ERRORS OF HIS MATERIA MEDICA.

OF the immense advantages which practical medicine may derive from testing the action of drugs on the healthy body, no one who has carefully considered the subject can doubt for a moment; and it owes Hahnemann much not only for having shown the importance of it, but likewise for the extraordinary energy which he displayed in carrying his proposal to test them into execution. Haller had already thrown out the idea that the action of medicines should be first tested on healthy individuals; and Störck, Alexander, and others had already made limited but important provings. But although Hahnemann cannot lay claim to originality either in making the proposal to prove drugs, or in actually proving them, it must be frankly admitted that he showed rare zeal in carrying out a gigantic undertaking, and in cultivating a seemingly repulsive and hitherto comparatively unknown branch of practical medicine. It is true that the form in which he published the results of his labours is radically defective, and numberless errors are scattered through his work; but notwithstanding its great and numerous defects, I consider the *Reine Arzneimittellehre*[1] to be one of the most important contributions that has yet been made to practical medicine. And our gratitude to Hahnemann and his followers for their provings will not be less when we recollect how little has been accomplished in this respect by allopathic practitioners, although many of them

[1] Hahnemann published the result of his labours in a work entitled *Reine Arzneimittellehre* (Pure Materia Medica), of which the first part appeared in the year 1811, and the sixth and last in 1821. I shall refer to it under the name of *Materia Medica.*

have recognised the numerous advantages that therapeutics may derive from that source. Several associations of allopathic physicians were indeed formed, in order to try the effects of medicines on healthy persons; but with the exception of those formed by Jörg, Rademacher's followers, and some physicians of Vienna, they did not accomplish anything. A number of associations of homœopathic physicians were likewise formed for the same purpose; but with the exception of the one formed at Vienna, which has given us the most complete and accurate provings that we possess, they accomplished comparatively little. Many individual practitioners, however, have made important contributions to the provings of the homœopathic *Materia Medica.*

The most striking defect of Hahnemann's provings is the artificial manner in which he has arranged the symptoms produced by drugs. Instead of arranging them in a sequential or natural order, he arranges them according to the parts of the body where they occur; so that we have a mass of symptoms recorded (sometimes amounting to nearly 2000 from one drug), without any attempt to show the order in which they were developed, or their connection with one another. Besides, this arrangement has the effect of surrounding with insurmountable difficulties the task of comparing the natural with drug diseases, in order to trace the resemblance between them. If the symptoms of any natural disease—pneumonia or typhus fever, for example—were arranged according to the plan adopted by Hahnemann for drug diseases, it is evident they could give us only confused and erroneous notions of these diseased states. That defect might be remedied by arranging the effects produced by drugs, or, to use the homœopathic phraseology, the symptoms of drug diseases, according to the method generally adopted in describing the symptoms of natural diseases in modern works on pathology.

Another great defect of his *Materia Medica* is this, that Hahnemann seldom informs us in what doses drugs were used in the provings, or how often or in what forms they were administered; nor does he give us any information respecting the age, sex, habits, or number of the provers. We are not informed whether or not a given symptom was remarked in one or more of the individuals who proved the drug. In short, for want of information on these important points, we have no means of appreciating at their proper value the effects ascribed to the action of medicines.

A striking feature of the homœopathic *Materia Medica*, and one that has often been the subject of remark, is the great number of subjective symptoms said to be produced by drugs. In Jahr's account of the action of cinchona, for example, the subjective symptoms are more than twice as numerous as the objective ones. As far as regards the comparative number of these two classes of symptoms, there is a marked difference between natural and medicinal diseases. In the greater number of the former—with the exception, perhaps, of some functional nervous affections, as hysteria—the subjective symptoms bear but a small proportion to the objective ones; whilst in the latter, according to the homœopathic *Materia Medica,* the number of subjective symptoms is almost invariably greater than that of the objective.

This circumstance of itself naturally suggests some doubts of the truth of the homœopathic principle, that all drugs can produce diseases similar to those which they cure. It cannot be denied that the proposition is erroneous, if we are to understand by it that they produce, when administered to healthy individuals, a limited series of symptoms similar to those which usually present themselves in different kinds of natural diseases. Dr. Madden expresses himself as follows: 'Forbes said truly, that their provings had no strict resemblance to any known diseases: they might display the same sphere of action, but they did not find among them the exact counterpart of any diseases.'[1] Dr. Dudgeon says: 'To have that, the symptoms developed by medicines must be analogous in their course to diseases; but this was not the case: the pathogeneses of medicines furnished them, as it were, with but fragments of the diseases.'[2]

If one drug can produce on healthy individuals a series of symptoms similar to those of any natural disorder, it should likewise be able, according to the homœopathic principle, to

[1] *Brit. Jour. of Hom.* vol. viii. p. 256.

[2] *Idem*, p. 257.

cure it. But it is generally acknowledged by homœopathic practitioners, that they cannot undertake the complete cure of any important disease with only one remedy; and in practice I have remarked, that skilful and experienced homœopathic practitioners changed their remedies pretty frequently during the progress of acute diseases. We may conclude, then, from what has been admitted by homœopathists, that drugs cannot produce diseased states resembling, both in the number and nature of their symptoms, natural maladies; and according to the homœopathic principle, if one remedy cannot cure a given malady, it cannot cause a similar one in a healthy individual. At these conclusions, I need not inform the reader, we formerly arrived when examining the truth of the principle, *similia similibus.*

It might be said that the expression, 'produces a medicinal disease similar to the natural one,' is to be understood only in this sense, that among the numerous symptoms supposed to be produced by a given drug, a certain number, taken individually, may correspond to those observed in a natural disease,—I say, taken individually, for the homœopathic *Materia Medica* has not yet succeeded in giving us descriptions of medicinal disorders, in which the symptoms are represented in their natural relation to one another, so as to admit of their comparison with those of natural diseases. A mere enumeration of isolated symptoms does not constitute a description of disease; and under these circumstances, it would only be by giving a forced and inadmissible meaning to the word that we could speak of similarity. Supposing the medicinal symptoms given in the homœopathic *Materia Medica* to have been accurately observed, a more correct expression, and one more in accordance with facts, would be, that most drugs produce a far greater number of symptoms than we observe in any natural disease; and that a larger or smaller number of these, taken separately, may correspond to those so observed.

However valuable some subjective symptoms may be, especially in functional disorders, it will readily be allowed by those who have reflected on the subject, that in many cases it is extremely difficult, or even impossible, to represent our sensations by words, or to exercise a proper control over the imagination in so doing. Even our most vivid sensations, unless occasionally renewed, soon become faint and indistinct, and at last fade entirely from the memory. Many of the subjective symptoms noted by the homœopathic provers of medicine, contain internal evidence that they are mere creations of the fancy. Every one can form an idea more or less clear of a burning pain; for there are few persons who have not frequently had occasion to experience the sensation of burning. But when a prover speaks of a tearing, lacerating pain, the question naturally arises, Has he ever had any of the tissues of his body torn? and if so, has he retained a distinct impression of the sensation that it caused? It is evident he cannot speak with accuracy of a sensation which he has never experienced, or which, if once or twice experienced, must have vanished so completely from his memory, that it cannot be distinctly recalled.

In the account given of the effects of cinchona in the homœopathic *Materia Medica*, one of the symptoms given is: 'Pain felt in the shoulder-blade as if it had been dislocated.' Had a dislocation ever happened to the prover? and if it had, how long did the distinctive character of the pain which it produced remain clearly impressed on his memory? In this and similar subjective symptoms, it is evident the prover must have drawn more or less on his imagination. Hahnemann was evidently of our opinion on this subject. In a note at p. 226 of the *Organon*, he remarks: 'The observer of others must always dread lest the experimenter did not feel exactly what he said, or lest he did not describe his sensations with the most appropriate expressions.'

In their provings, homœopathic practitioners have attempted to characterize about thirty modifications of the sensation of pain by such epithets as aching, boring, rending, drawing, tingling, gnawing, incisive, jerking, pressing, piercing, crawling, contractive, etc.; but their provings would be far more valuable if, instead of making impracticable attempts at precision, they diminished considerably the number of epithets, and characterized only the more familiar modifications of pain, as burning, throbbing, pricking, etc. As subjective symptoms, then, are apt to be incorrectly described, they must be accepted with caution.

On reading over the provings of drugs recorded in the homœopathic *Materia Medica*, one is struck by the great number of symptoms which each is said to have produced. In this respect there is a marked contrast between the results obtained by trials of drugs made by homœopathic, and those made with the same drugs by allopathic practitioners, as Jörg, Rademacher's followers, and the proving association of Vienna. I have given at full length Jörg's trials with sulphur and cinchona, in order to show how limited were the number of symptoms reported by him, compared with those ascribed to the same medicines in the homœopathic *Materia Medica.* It has been said that the great number of symptoms reported in the provings contained in it can be easily accounted for by the great number of individuals who took a part in them. I doubt, however, if this explanation can be considered satisfactory; for, in the first place, we know little or nothing about the number of individuals who took a part in these provings; and, in the second place, it would imply greater variety in the action of drugs on different individuals than observation warrants us to admit.

There are two circumstances which, I think, may partly account at least for the greater number of subjective symptoms remarked in medicinal than in natural diseases, and likewise for the great number of symptoms in general said by homœopathists to be produced by drugs in healthy individuals. No one could expect the provers of drugs to use such large doses as would cause any sensible physical change in the state of their organs—for example, inflammation; or, in other words, as would produce more than mere functional derangement (and that, too, not very severe) of their organs,—a condition, consequently, in which the subjective bear a larger proportion to the objective symptoms than usually happens in natural maladies. Again, the principle inculcated by Hahnemann on the provers of medicines, 'to watch carefully during the provings for any change or modification either of their mental or of their physical states,' was eminently calculated, from the known influence of the attention when directed to our sensations, to furnish a very great number of symptoms.

The defects which we have pointed out in the homœopathic *Materia Medica* might be removed by re-proving the medicines on a more scientific plan than has yet been adopted. But defects of a far more serious nature, arising from the inaccuracy and impurity of many of the provings (of which the results were embodied with the homœopathic *Materia Medica*), have been pointed out by many distinguished homœopathic physicians. To enter into a detailed examination of this point would occupy too much space in a work like the present. I shall therefore only give the opinions of some of the medical men who appear to have given this important subject a serious and impartial consideration.

In a number of one of the homœopathic journals, Dr. Roth of Paris says: 'Gross was right; not hundreds, but thousands, of symptoms figure in the *Materia Medica* which do not belong to the medicinal action of the tested drugs.'[1] In another volume of the same work, he expresses himself in the following manner: 'The nominally but not really pure *Materia Medica* is a mixture of the greatest errors.' 'The pure *Materia Medica* is not pure, for it contains about 2000 false quotations; it is not pure, for it contains many thousands of badly observed symptoms from sick persons.'[2] Hempel, a homœopathic physician, at p. 131 of his *Organon*, says: 'The provings or drug symptoms which make up the homœopathic *Materia Medica*, so far from constituting a series of incontrovertible facts, is, on the contrary, liable to the grave and well-founded charge of being in a great measure a tissue of fallacious illusions, misapprehensions, absurdities, and childish observations.' Even Jahr, whom no one will accuse of being too ready to admit the deficiencies of homœopathy, remarks: 'I, who with pen in hand have gone through the whole *Materia Medica* more than once, and made a severe critical comparison not only of the various provings with one another, but likewise of the symptoms observed by the same provers with various medicines, could say something about the trustworthiness of individual provers and their provings.'[3]

In a review of Hempel's translation of Hahnemann's work on *Chronic Diseases*, the reviewer justifies Hempel's omission of 205 symptoms from the account of the proving of sarsaparilla

[1] *Hom. Vierteljahrsschrift*, vol. xii. p. 65.
[2] *Idem*, vol. xiv. p. 165.
[3] *Allgem. hom. Zeitung*, vol. x. p. 226.

by a person under the signature 'Ng.' as untrustworthy; and in the same work he gives the following extract from a note by Hahnemann on magnesia carbonica: 'The name of the original prover is not given, but they (the symptoms) bear the stamp of having their origin in the ever ready symptom-manufactory of Ng.' So writes Hahnemann of this individual, at the same time that he makes use of his provings, and that very largely. Of the forty-seven medicines treated of in his work on *Chronic Diseases*, twenty were proved by the person on whose provings he throws so much doubt. Again, in a note on alumina, speaking of the same person, he says: 'He made use of his provings only on the understanding that he conducted his experiments like an honest man.'[1]

I believe Ng. was no other than the surgeon Cajetan Nenning, of whom Dr. Roth, in his *Studies*, gives us some curious information. His opinion of the worth of Nenning's provings is similar to that of the author of the review from which we have just quoted. He says the symptoms furnished by Cajetan Nenning cannot be allowed to remain in the homœopathic *Materia Medica* under any circumstances. Nenning proved thirty-eight medicines altogether, and furnished 11,447 (?) symptoms to the *Annalen* of Hartlaub and Trinks, to Hahnemann's *Pure Materia Medica*, and to his work on *Chronic Diseases*. Ng. confesses that he did not prove one of the drugs on himself, and allows that his provings were not made with proper precautions.

In the article to which we have already referred, in vol. xx. p. 690 of the *British Journal of Homœopathy*, the reviewer says that 'Langhammer, who rivals Nenning in the number of provings he professed to make, is not a bit more to be depended on than Ng.' Dr. Roth says[2] we only require, as Gross has done for Langhammer, to compare with one another the symptoms noted by the same person in proving different medicines, to be convinced of their worthlessness. Langhammer proved 52 medicines, and furnished 1800 symptoms to the homœopathic *Materia Medica*.

Von Gersdorf proved 10 medicines, and furnished 1716 symp-

[1] *Brit. Jour. of Hom.* vol. xx. p. 688.
[2] *Hom. Vierteljahrsschrift*, vol. xii. p. 65.

toms. According to Dr. Roth,[1] most if not all the symptoms contributed by him are individual, and not medicinal; and although he did not consider them fabricated, yet for reasons assigned he regarded them as useless; and Dr. Helbig was of the same opinion. At page 395 of the same vol., Dr. Roth informs us Von Gersdorf proved zinc on two different occasions. An account of the first trial is given in the 2d No. of 6th vol. of the *Archiv;* and of the other in the 5th vol. of Hahnemann's work on *Chronic Diseases:* 'If we compare the two trials, we find less similarity between them, even with respect to ætiological relations, than between zinc and any other of the ten drugs proved by him.'

Frederick Hahnemann, son of the founder of homœopathy, proved 33 drugs, and furnished 938 symptoms. Dr. Hartmann criticises his provings severely; and Dr. Roth, who enters into a detailed examination of them, considers them, in general, untrustworthy, and informs us that many of his supposed pure symptoms were observed only in disease.[2] Homrada's provings, like those of Nenning, were paid for, and are as little to be relied on as those of the latter person. As he informs us, they were chiefly made on peasants during the winter months, when they had little to do. He proved chiefly mineral waters; but of sabadilla he has furnished 65 symptoms, and of moschus 22.

We may here notice some trials made by Greding, a contemporary and friend of Störck, with veratrum, hyoscyamus, and other two medicines. They were made on epileptics and lunatics, and the medicine to be tested was generally mixed up with others. Hahnemann has incorporated 172 symptoms from the trials with veratrum in his *Materia Medica.* I do not know how many were taken from the trials with the other medicines. Dr. Gerstel says these symptoms can be unconditionally excluded from the homœopathic *Materia Medica,* and Dr. Roth is of the same opinion. The latter physician sums up the result of his critical examination of the provings of Hahnemann's *Materia Medica,* and of his work on *Chronic Diseases,* in the following words: 'Altogether, we have the first contingent of 16,140 errors, that must of necessity be excluded from the

[1] *Hom. Vierteljahrsschrift,* vol. xii. p. 397.
[2] *Idem,* p. 395.

Materia Medica; and there are many more thousands which can be pointed out in future "Studies."'[1]

I shall here quote some remarks by Dr. Langheintz, of Darmstadt, on the untrustworthiness of some of Hahnemann's own provings—those of opium and musk. After a most elaborate examination of his provings of these drugs, he says, speaking only of those symptoms which he had the means of checking: 'A considerable number of the symptoms were, it is well known, observed on sick persons after they had swallowed mixtures containing opium. Besides, many symptoms were taken from *Compendiums of Materia Medica,* without its being shown that they were the result of observations on the healthy, and not generalizing abstractions from patients who had made use of opium; that, in short, the number of symptoms observed on the healthy must be much the smaller, and from circumstances we must conclude that Hahnemann's proving of opium is incomplete and impure.' With regard to the proving of musk in the *Materia Medica,* the same author adds: 'Of all the symptoms ascribed to musk in that work, only very few can be considered conditionally admissible: the rest of them should be forthwith rejected.'[2]

Eminent homœopathic practitioners had repeatedly pointed out the errors and impurities of the homœopathic *Materia Medica,* and the consequent necessity for re-proving its drugs, and for correcting the numberless false quotations which it contained. That task was at length undertaken by the homœopathic proving association of Vienna, which carried out its object with great energy and judgment for several years. It succeeded in re-proving a considerable number of drugs; but since Dr. Wurmb's death its labours have been brought to a close, and at a meeting of its members, which was held in the autumn of the year 1865, the resolution to discontinue the proving of drugs was unanimously adopted for satisfactory enough reasons. I have frequently seen it stated in works on homœopathy, that the re-provings of that association completely confirmed the accuracy of some of Hahnemann's provings. I take the statement for what it is worth, and place it beside the

[1] *Hom. Vierteljahrsschrift,* vol. iv. p. 172.
[2] *Idem,* p. 249.

results obtained from a critical examination of some of the provings contained in his *Materia Medica.*

I shall conclude my remarks by quoting the words of one of the most eminent homœopathic physicians of the present day, Dr. Veit Meyer, of Leipzig: 'How often have we taken up Hahnemann's *Materia Medica,* with the intention of studying some drug, and making ourselves familiarly acquainted with it, and as often reluctantly closed the book with disgust!' 'How can we expect it from young homœopathic practitioners, when our *Materia Medica* is in such a state of chaos?'

PART II.

CHAPTER I.

OF WHAT DISEASES THE HOMŒOPATHIC AND NON-HOMŒOPATHIC TREATMENT WILL BE COMPARED—ABSENCE OF FIXED PRINCIPLES IN THERAPEUTICS—DISCUSSIONS ON THE TREATMENT OF RHEUMATISM AT THE FRENCH ACADEMY OF MEDICINE.

I SHALL now proceed to compare the results obtained in the treatment of disease by medical men of the old school with those given by homœopathic practitioners. It must be confessed, however, that as far as regards properly detailed cases, the data furnished by the latter leave much to be desired, although some of their reports will bear a comparison with the best of those of the old school. Any one who reads Dr. Tessier's cases of pneumonia or of cholera can follow satisfactorily their progress, and judge for himself of the accuracy of their diagnosis.

The only objection that Dr. Valleix, after a rather hypercritical examination of those of pneumonia, has been able to bring against them is this, that he considers three or four of them to be cases of capillary bronchitis, and not of pneumonia. But even if his opinions were well founded, it would not affect the value of Dr. Tessier's recoveries; for capillary bronchitis is at least as serious a disease as pneumonia. It is a pity that other homœopathic practitioners have not imitated his example, and carried out their therapeutic researches in as satisfactory a manner as he has done.

In making trials to ascertain the comparative merit of two methods of treatment in any given disease, it is evident that the more closely the cases contained in the two groups resemble one another, the less risk will there be of drawing erroneous conclusions. It would contribute much to ensure accuracy of observation, if the diseases of which the treatment is to be compared were of such a nature that their diagnosis could be satisfactorily established, and if medical men were familiar with their general course. It would likewise be desirable that several of them should be diseases of which some of the most prominent and persistent phenomena present themselves to our view, as erysipelas, scarlet fever, or small-pox, and in which, consequently, we could remark more accurately any change or modification which the treatment might produce. An important circumstance which should not be overlooked in such an investigation is this, that the cases of which the treatment is to be compared should have occurred about the same time and in the same locality, on account of the great variations in the mortality of a disease, even with similar treatment, as it occurs in different places, and even in the same place at different periods. Unfortunately I have scarcely been able to find any properly detailed cases, in which the conditions to which I have referred are found combined.

To be able to judge as accurately as possible of the comparative utility of homœopathic and non-homœopathic treatment, I have endeavoured to procure the most trustworthy hospital reports of both methods. For obvious reasons, I have not made use of reports of private practice. The diseases of whose treatment I shall compare the results are rheumatism, intermittent fever, cholera, typhus fever, and pneumonia.

Before proceeding further, I intended to have given a short sketch of the present state of therapeutics, in order to show that it has no fixed principles, and that the treatment of almost all important diseases presents a constantly changing and frequently conflicting variety. But the unsatisfactory state of our art has been so generally and so explicitly admitted by its most distinguished professors, that it would be useless to make a formal examination of the subject. To show, however, that the general opinion is but too well founded, and at the same time to give a clear and vivid idea of the present state of therapeutics, as far at least as regards one important disease, acute articular rheumatism, I will present a summary of the discussions which took place on that subject at the Academy of Medicine at Paris in the year 1850, and which occupied its attention during five successive meetings.

I select these discussions to illustrate the truth of my statement, as much on account of the publicity given to the expression of its opinions by that distinguished body, as of the great influence which it exercises on medical science.

There can be no doubt, however, from the opinions held on the subject of rheumatism in the medical works published in different countries of Europe, that if the most eminent physicians of any other country were to meet and discuss the nature and treatment of that disease, the variety of their pathological views and of their plans of treatment would rival that of the French physicians. I may add further, that the views of practitioners respecting the treatment of the other diseases which I intend to examine, with the exception perhaps of intermittent fever, will be found as various and conflicting as those regarding that of acute rheumatism: indeed, the same may be truly said of all important diseases. The discussions which formerly took place at different periods, in the same Academy, on the treatment of typhus fever, cholera, pneumonia, and other maladies, as well as the opinions advanced in works on therapeutics published in different countries, completely support what I have stated.

Until about thirty years ago, the general treatment of inflammation might have laid claim to something like fixed principles; but since that period, the results of expectant and homœopathic practice, as well as of the restorative treatment proposed by Dr. Hughes Bennett, have clearly shown the fallacious nature of the views so long entertained on that subject. In short, there is no important disease of which the therapeutics may not be truly and briefly summed up as a collection of various, often directly opposite, plans of treatment. I do not mean to say, however, that the use of drugs does not often produce most beneficial results; I merely affirm that at present therapeutics have no fixed principles.

In the year 1850, Dr. de Chilly, physician of the Hospital of Vaucouleurs, presented a paper to the Academy of Medicine of Paris, entitled *Du Traitement du Rheumatism Articulaire aigu par les Vesicatoires volants.* Dr. Martin Solon was appointed to report on it. I shall give a curtailed account of Dr. de Chilly's views on the nature and treatment of acute rheumatism, and of the discussions which took place on these points when the report was read.

Dr. de Chilly says rheumatism is as little an inflammation of the articulations, as small-pox or measles are inflammations of the skin. Bleeding attacks only the febrile part of the complaint: it does not act on the cause of the disease. The want of efficacy of bleeding, and of the nitrate of potash, as well as the danger that he had seen caused by the use of the sulphate of quinine, made him seek for some other method of treating the disease. He found blisters, applied to the entire surface of the affected articulation during the acute stage of the disease, to be the most efficacious. When the disease is driven from one articulation, it must be attacked in whatever one it fixes itself. A considerable number of blisters may be required for the cure. In one case, thirteen were applied at six times. It got well in eighteen days; but in others, the treatment lasted much longer.

M. Solon, the reporter, said the method of treatment of Dr. de Chilly is an important one, and deserves to be tried, as it might present advantages that we should expect in vain, perhaps, from other methods; and it might advantageously replace them in cases in which the strength was much impaired, or in which the state of the digestive organs contraindicated the use of counter-stimulants internally.

When the report was read, M. Rochoux declared, 'that to say acute rheumatism is not an inflammation, was an evident absurdity. It is the type of inflammation, on which blisters can only have a most injurious influence. I am consequently far from regarding, like M. Solon, sulphate of quinine, nitrate of potash, and large bleedings, as equivalent remedies. I do not attach much importance to the counter-stimulant plan, which, considered physiologically, really deserves to be hissed, like many other systems which have had a momentary vogue.' M. Bouilland declared that acute articular rheumatism was one of the most formidable diseases with which he was acquainted. Pity on the patient who suffers longer than fifteen days from it. Bleeding *coup sur coup* is the only sure and certain remedy, and cures even the most severe cases in less than a week. The important point is to watch the occurrence of complications, especially of the heart. With blisters alone we cannot cure acute articular rheumatism: they may be useful auxiliaries, but their use must be preceded by more or less frequent bleedings. Sulphate of quinine and nitrate of potash he condemned on account of their inertness. M. Solon replied that he considered bleeding only as an accessory remedy. As principal remedies he used sometimes sulphas quiniæ, sometimes nitras potassæ, with which he cured the most intense and acute cases in less than five, eight, or ten days,—generally between the fifth and sixth day.

With the exception of MM. Rochoux, Bouilland, and Piorry, the academicians seemed little disposed to admit acute rheumatism to be simple inflammation. M. Gerdy decidedly opposed the opinion of Rochoux and Bouilland, that it is an inflammation. The extreme mobility of the disease showed, he said, that this was not the case.

M. Grisolle was completely opposed to all that Rochoux and Bouilland said. He went further than M. Gerdy, and said that inflammation was not even one of the elements of rheumatism—it was only a complication. Thus, when adynamic symptoms occur in pneumonia, they completely change the indications in a disease essentially inflammatory, and which must be treated without preoccupying ourselves with its primary nature. He was quite opposed to repeated bleedings: he thought the cardiac complications which so frequently occurred in Bouilland's cases were owing to excessive depletion. He cited Monneret and Legroux, who were compelled to give up the plan of bleeding *coup sur coup* in rheumatism, on account of the dangers resulting from it. Besides, it did not lessen the duration of the disease, whether it was performed *coup sur coup*, or moderately at considerable intervals. The case of a female patient of M. Chomel's is published in the *Lancette Française*,[1] to illustrate the inutility of repeated bleeding in rheumatism. Twelve pounds of blood had been abstracted; the disease appeared to terminate in twenty-five days. A few days later the rheumatism returned for a fortnight longer. Bouilland, continued M. Grisolle, gives a week

[1] No. for Oct. 1835.

as the average duration of an attack of acute rheumatism treated *coup sur coup;* but Rochoux, who likewise bled copiously, affirms that the average duration of acute rheumatism is forty days.

M. Rochoux replied, 'that the mobility of inflammation is not peculiar to rheumatism; we witness it in inflammation of the serous membranes: in short, the success of depletion in this complaint is a proof of its inflammatory nature.' M. Bouilland said he did not pretend to localize rheumatism, which is a general disease, in which the whole mass of the blood participates: it presents, in every respect, the phenomena common to inflammation.

M. Grisolle renewed his attack on Bouilland. After an impartial examination, he said, of the cases of acute rheumatism which Bouilland had published, he found, counting correctly, that the disease, as treated by him, lasted on an average not one, but three weeks, which Chomel has shown to be the average duration of cases of acute rheumatism left to themselves. M. Piorry now assured the Academy that he had not remarked any favourable modification of the disease from the use of the remedies that had been recommended. He rejected them all, and used only repeated bleedings. He said he had employed them before M. Bouilland did, and asserted that he had been more fortunate than that physician. Three and a half days was the average duration of his cases; but his statement seemed to fall on incredulous ears.

M. Malgaigne now came forward. He reproached physicians much for not attaching sufficient importance to the natural history of the complaint, and for attending too little to its local effects, whilst they give too much attention to the general state of the patients, who are not unfrequently sent out of the hospital without being completely cured—with their joints stiff. The proof that they have not been cured is the fact that they have subsequently come to the surgical wards to get rid of their ailments.

M. Parchappe declared that the disease is a general affection, a *pyrexie,* of which the nature is unknown. It has a definite duration, and must pass through certain phases; consequently no active treatment can prevent it from running its course.

We must employ the expectant method, then; that is, leave the disease to itself. Chomel had already said: 'Let us confess with pain, that art has no certain means to arrest, or even abridge, the duration of rheumatism.'[1] Parchappe — rather inconsistently, however — admitted the utility of bleeding. Bouilland now stated that the treatment of rheumatism, to be satisfactory, must be commenced as early as possible, at least before the end of the first week. He brought forward 39 new cases, cured in five to six days of average treatment; or from ten to twelve days, counting from the beginning of the disease; or, with the time of convalescence included, twenty-six days, as the total average duration,—a statement that justified Grisolle's former reproach.

M. Tanchoux assured the Academy that the cold-water cure had given him the most numerous, durable, and complete cures. M. Levrat said he had employed at different times bleeding *coup sur coup*, purgatives, nitrate of potash, and sulphate of quinine. These means were sometimes successful; but more frequently they failed. For the last fourteen years he had treated acute rheumatism with purgatives and preparations of colchicum, associated with sulphate of quinine and extract of opium. His success has been such, that he did not hesitate to consider the treatment as quite specific.

M. Bouchardat now took a part in the discussion, and made some remarks on the different methods of treatment that had been recommended. He thought that acute rheumatism depended on a peculiar state of the organs. It appeared to him to be now established, 'that large bleedings at short intervals had no injurious influence on the future health of the patients; and that they are efficacious in preventing the serious complications with which patients suffering from acute rheumatism are threatened.' Monneret and others, however, have shown that copious blood-letting gives rise to chloranæmia, and renders any treatment requisite in case of relapse very difficult. Legroux even says that they favour the development of cardiac complications; and Gouzée and Louis are of the same opinion. Beau has endeavoured to show that they give rise to hypertrophy of the heart.

[1] *Clinique*, vol. ii. p. 274.

'Sulphate of quinine, properly employed,' continued M. Bouchardat, 'is as efficacious as any other remedy; but its administration is not so easy as might be supposed. In alterative doses its efficacy has never been marked in acute rheumatism; in large doses its poisonous influence cannot now be doubted, for unfortunately we have many well-established cases of death directly caused by its use in large doses in the treatment of acute rheumatism. Digitalis, squills, and colchicum modify the progress of the complaint, by causing disturbance in the system, without their superiority to quinine being established; and as their administration is surrounded by greater difficulties than that of the last-named medicine, he would not say anything more about them.' 'The use of the nitrate of potash in large doses is of considerable utility, but it must be given in divided doses much diluted, or it may cause fatal accidents. While using it or quinine, great attention must be used to see that they are properly eliminated in the urine. In certain cases opiates may be useful in lulling pain, and advantageously influencing the progress of the cases; but their use must not be continued too long, on account of their injurious influence on nutrition and digestion. M. Vergne, in his *Thesis*, mentions several cases of acute rheumatism which proved fatal at the Hotel Dieu, from the use of excessive doses of opium, and others in which its effects were merely injurious.'

The long discussion on acute rheumatism was at last brought to a close by a few remarks from M. Levy, who succeeded in bringing it back to the subject of blisters. He doubted the utility of blisters, at least as a specific in this complaint. M. Chilly was wrong in wishing to generalize a treatment which was applicable only to exceptional cases.

M. Solon, in closing the discussion, said: 'Sulphate of quinine must not be used when cerebral congestion is present. Nitrate of potash, the remedy best suited to the disease, and which acts in a special manner on the blood by making the fibrinous element disappear, should not be used when there is gastro-intestinal complication. Blisters are useful in particular cases in which the other remedies cannot be used. In short,' said M. Solon, 'the treatment of acute rheumatism is improving, and by persevering in our present path it will improve still more.' 'At present, it would be of the utmost importance to compare the different methods of treatment with one another.' M. Solon's last phrase looks like a mystification of his audience. The French say, '*Du choque des opinions jaillit la verité.*' But what truth has been elicited from this long discussion, which shows in what a hopeless state practical medicine is at present, at least as far as regards the treatment of acute rheumatism? Incessant contradiction! No fixed principle!

About five years after this discussion took place, a new medicine, veratrine, came into vogue. It was tried at the clinique of M. Trousseau. In giving an account of it, M. Bouchut remarks, 'that nitrate of potash has never been able to arrest the course of acute articular rheumatism.' M. Solon and others considered it the most trustworthy medicine in that disease. Sulphate of quinine, with the results of which he was much pleased (Bouilland and Piorry denied its efficacy), presents the disadvantage of being expensive, and sometimes dangerous when not administered circumspectly. Unfortunately, says Bouchut, veratrine is not well borne by all patients: there are some who cannot bear it at all. He gives the results of the treatment of nine cases, and these results have been confirmed by new ones. In four of them it seemed to cure rapidly; but the cases were not severe. In one of a gouty nature it was of no use. In two very acute cases it was used during thirteen and sixteen days respectively, without influencing at all the progress of the complaint; symptoms of endocarditis developed themselves in both cases. In other two it was impossible to continue the use of it, on account of the want of tolerance of the drug; but 5 to 25 millegrammes of veratrine a day can never cause death, as has happened from the use of quinine. In short, M. Bouchut placed it in the first rank of remedies for acute articular rheumatism.

The remarks of Arran will enable us to appreciate these statements at their proper value. In an article on pneumonia and articular rheumatism, he says: 'Almost all the patients, after taking 5 to 15 millegrammes of veratrine, had sickness, vomiting, with a sensation of burning in the œsophagus and stomach. In some cases nausea and vomiting succeeded each other so rapidly, that there was scarcely an interval of five to ten minutes between them. The vascular, respiratory, and nervous systems were most severely affected; the pulse was rapidly brought down from 100 to 40; the respirations lessened to 6 or 8 in a minute; and in all cases animal heat was much diminished. It might be said,' adds Arran, 'that we should stop when dangerous symptoms appeared; but we are advised to push the use of it so far as to cause a marked depression in the most important functions of the economy.'[1] In short, it may be said, the patient requires to be well poisoned!

[1] *Bulletin Therapeutique*, Nov. 15, 1853.

CHAPTER II.

I.—NON-HOMŒOPATHIC TREATMENT OF ACUTE ARTICULAR RHEUMATISM.

I SHALL now give some results (taken indiscriminately) obtained by non-homœopathic treatment of acute rheumatism, not with the intention of comparing them formally with those of homœopathy, but simply to show that in time and place the former method can give results nearly as satisfactory as those of the latter; and to show, moreover, that they are better, the less active or more expectant the treatment employed.

After the conflicting accounts respecting the treatment of acute articular rheumatism given in the last chapter, let us see what Dr. Gouzée,[1] head physician of the Hospital of Antwerp, and others, have said of the purely expectant treatment of that disease: 'Great success has been lately ascribed to the most violent and discordant plans of treatment. Tartar emetic, nitre, bleeding, opium, iodide of potassium, sulphate of quinine, have been employed in enormous doses. It has been said that sulphate of quinine in Rassorian doses has sometimes missed the mark, and struck the patient instead of the disease. For a long time past I have employed the expectant treatment, and not a year passes by that I have not reason to be astonished at the facility and rapidity of my cures, when I consider the trouble that other physicians give themselves to obtain the same results, if they do obtain them.'

The results of his practice since 1843 have confirmed the truth of the preceding statements; and Dr. Delawalsche, considering the present divergence of medical opinion on the subject, thought it his duty to collect and publish several cases of acute articular rheumatism treated at the clinique[2] of Dr.

[1] *Archives de Médecine Belge*, Jan. 7, 1844.
[2] *Gazette des Hôpitaux*, Jan. 30, 1853, p. 364.

Gouzée by purely hygienic and dietetic means. He gives a detailed account of six taken indiscriminately from those treated, and he draws the following conclusions :—

1. Acute articular rheumatism has a natural tendency to terminate in the course of the first or second week.

2. Treated expectantly, with the aid of some hygienic or dietetic means, it continues its course without accident or danger; and terminates as soon, if not sooner, than when treated by active remedies.

3. It is by no means proved that the active methods of treatment which have been recommended are useful, or even harmless.

It is much to be regretted that neither Dr. Gouzée nor Dr. Delawalsche have told us the number of cases treated, or how many of them recovered. Their important statements lose much of their value for want of a little more precision. The results obtained in London by Dr. Chambers with the purely expectant treatment, although on a very small scale, seem to confirm the statements of Gouzée and Delawalsche, with the exception of what relates to the duration of the disease, which lasted much longer in Chambers' cases than in those of Gouzée and Delawalsche.

Drs. Gull and Sutton have published in the number of the *Medical Gazette* for Jan. 16, 1869, an interesting account of 25 cases of acute articular rheumatism, which were allowed to run their course without the use of drugs. The average duration of the complaint from the beginning till the end of the disease was seventeen days. The authors of the article say that the heart rarely became affected, if first attack, and uninfluenced by drugs; and that no plan of treatment seems to have any great advantage in shortening the disease.

Dr. Smoler[1] gives the result of the treatment of 200 cases of rheumatism in Professor Halla's clinique at Prague. The average duration of each was nineteen days; but as Dr. Smoler places in the same category acute articular rheumatism, muscular rheumatism, rheumatic neuralgia, etc., we cannot compare his results with those obtained from acute articular rheumatism alone. The mortality was 6 per cent.—5 per cent. for men, 7

[1] *Ueber die Dauer einiger acuten Krankheiten : Wiener Zeitschrift*, Band xviii. p. 151 (1862).

per cent. for women; or, deducting six fatal cases of puerperal fever that occurred during the rheumatic attacks, 1 per cent. There were complications in 47 per cent. of the cases. The local treatment consisted of the application of compresses of cold water, often changed, to the affected parts; and even to the chest or cardiac region, when pectoral or cardiac complications occurred. Internally, infusion of digitalis or ipecacuanha was used with or without opium; when heat great, the body was washed with vinegar; when collapse was threatened, sulphate of quinine, etc., were given.

Dr. Roth[1] gives an account of 79 cases of acute articular rheumatism, treated at the Julius Hospital, Wurzburg, during three years, 1857–1860. Of the 79 cases, 3 died, or 3·7 per cent. A third of the cases were complicated; of cardiac complications alone, there were 18·9 per cent. The treatment was chiefly symptomatic, and that generally not very active. He most frequently employed nitre at the commencement of the disease, and digitalis when cardiac complications were apprehended. Morphine was of great use. Veratrine had not the slightest influence on the affected articulations. Quinine was extensively used in large doses; but Dr. Roth did not remark any favourable results from it, with the exceptions of the temporary lowering of the pulse and heat. Venesection was only once employed: it had no effect on the progress of the case.

In none of the three fatal cases was acute rheumatism the direct cause of death; and had he classed his cases as some homœopathic practitioners (Dr Wurmb, for example, who frequently classed his fatal cases under the head of the complication that was the immediate cause of death, as we shall afterwards see) have done, there would not have been any mortality. The first fatal case was that of a young man, aged 17, in which the symptoms of acute rheumatism had almost entirely disappeared, when periosteal abscesses began to form on the left tibia and femor. He was therefore transferred to the surgical wards, where he died a long time afterwards. The second case was that of a girl of 25 years, with double pleuro-pneumonia and endocarditis. The third case was a maid-servant, aged 22 years, who suffered from insufficiency of the mitral valve of long-

[1] *Würzb. med. Zeitschrift*, vol. iv. p. 277 (1863).

standing. During the attack of rheumatic fever she got endo- and peri-carditis and double pleuro-pneumonia. Dr. Roth mentions in his paper that Dr. Lebert lost only 3 per cent. of 230 cases of acute rheumatism, and that his fatal cases all died of complications with cardiac inflammation, or affections of the nervous system. The average duration of the cases was 23·1 days. The same authority states that, of 108 cases treated by Dr. Wunderlich, 2 only, or 1·8 per cent., died: in both pyæmia was the cause of death. The average duration of his cases was 23·6 days.

At the meeting of the British Medical Association held at Bristol, Dr. Chambers, of St. Mary's Hospital, London, read a very interesting paper on acute rheumatism, which was published in the *British Medical Journal* for 1864. Since the year 1851, Dr. Chambers had treated 243 cases of acute rheumatism, of which 4 died, but not from simple rheumatism. Two of them died from cardiac inflammation; two from sloughing sores on the back. The mortality, then, was about 1·64 per cent.

Of the 243 cases—

26 were treated with ʒi. doses of nitr. potas. ter in die, of which 4 died.
144 ,, with ℈i. doses of bicarb. potas. om. bihorio.
33 ,, with smaller quantities of ditto.
32 in various other ways.
11 used no drug.

Of those treated with	nitr. potassæ,	average duration	40 days.
,,	carb. potassæ,	,,	34 ,,
,,	in smaller quantities,	,,	40 ,,
,,	without drugs,	,,	30 ,,

Of 180 bedded in blankets, none contracted pericarditis or died.
Of 63 ,, in sheets, 4 died.

The cases in which no medicine was used gave the most satisfactory results; but the number observed was far too small to allow us to draw any certain conclusion from them.

II.—HOMŒOPATHIC TREATMENT OF ACUTE ARTICULAR RHEUMATISM.

Let us now see what success the homœopathic treatment has had in acute rheumatism. Unfortunately, we have not a sufficient number of satisfactory data to enable us to give a decided opinion on this point. We may compensate a little, however, for that deficiency, by citing afterwards the opinions of one of the most distinguished homœopathic physicians of his day on the value of homœopathic treatment in cases of acute rheumatism. In the 14th vol. of the *British Journal of Homœopathy*, a table is given of the cases treated by Dr. Fleischmann in the Homœopathic Hospital of Gumpendorf, from the year 1834 till the year 1855. We find, under the head of rheumatic fever, 1417 cases, all of which recovered, except 1· that remained under treatment; under that of acute and chronic rheumatism, 759 cases, of which 756 recovered, 2 died, and 1 remained under treatment; under the head of inflammation of the joints, 888 cases, with 7 deaths from miliary fever. I have no hesitation in saying there must be some mistakes in Fleischmann's statistics of rheumatic affections, or he must have classed them in a manner peculiar to himself.[1] 1417 cases of rheumatic fever without a single death will find many unbelievers, especially when it is found that in about half that number of cases of acute and chronic rheumatism there are two deaths, and in 888 cases of inflammation of joints seven deaths caused by miliary fever.

The results of the homœopathic treatment of acute rheumatism by Drs. Wurmb and Caspar are given in a much more satisfactory manner. They treated 69 cases of acute rheumatism (20 males and 49 females), of which 22 were muscular and 47 articular. Only one of these patients died suddenly; but from the brief history given of the case, it is impossible to say whether it was one of articular or of muscular rheumatism. As they speak of the fever having ceased, we may presume it was one of articular rheumatism, which would give a mortality of 2·12 per cent. for articular rheumatism, and of 1·45 per cent. for mixed cases of articular and muscular.

Compared with Smoler's mixed cases, the mortality of those

[1] In a note to one of the numbers of the *Brit. Jour. of Homœopathy*, one of the editors remarks, 'that even the bitterest opponents of Dr. Fleischmann have always admitted him to be a proficient in physical diagnosis.' I may here remark that one or two of his homœopathic confrères, who were quite competent to form an opinion on that point, did not consider his skill in diagnosis to be very remarkable. See further, under Homœopathic Treatment of Pneumonia.

of Wurmb and Caspar was less than in the former by 1·56 per cent. The mortality of their cases of articular rheumatism was less than that of Roth's by 1·6 per cent., and than that of Lebert's by ·88 per cent.; and greater than that of Wunderlich's by ·32 per cent., and than that of Chambers' by ·48 per cent.

I shall now give an extract from a printed report[1] which I have lately received. It is the only one I have been able to procure, in which the general condition of the patients affected with rheumatism was as nearly as possible the same. The results of the homœopathic and non-homœopathic treatment of these cases can therefore be fairly compared. The Gumpendorf and Leopoldstadt Hospitals at Vienna are both under the management of the Sisters of Mercy. In the latter there is a section in which the sick are treated allopathically. As in most of the allopathic hospitals of Vienna, the treatment is more or less expectant or palliative.

The report begins from the year 1858, and extends over a period of seven consecutive years for the Gumpendorf, of six for the allopathic section of the Leopoldstadt, and of five for the homœopathic section of the same. The cases, including muscular and articular rheumatism, are arranged under the head of rheumatism, without any explanatory remarks.

In the Gumpendorf Hospital, 1403 cases were treated, with 4 deaths, or 1 death in 350 cases.

In the homœopathic section of the Leopoldstadt Hospital, 743 cases were treated, with 2 deaths, or 1 death in 371 cases. In the allopathic section of the same hospital, 448 cases were treated, without any deaths; so that the results are in favour of the allopathic treatment.

I shall now quote the remarks of Drs. Wurmb and Caspar,[2] to which I referred: 'We homœopathists unfortunately have likewise no reason to be satisfied with the present state of the treatment of rheumatism, for it is certainly not a satisfactory one. We do not doubt that many of our confrères have made very fine cures; but still we do not hesitate to affirm that, as

[1] The reports have been printed at Vienna by order of Ernest Max Hurez, Superior of the Order of the Sisters of Mercy.

[2] *Homœopath. Klinische Studien* (Vienna 1852), p. 224.

regards the general treatment of rheumatism, nothing has been accomplished up to the present day; and homœopathists have this disadvantage in comparison with their opponents, that they are not aware of the fact.'

'Muscular rheumatism is not an important disease. It lasts sometimes longer, sometimes shorter; so that it is always a difficult point to determine, in any particular case, if nature or art has made the cure. Several cases presented themselves, which lasted from their commencement till their complete disappearance, 4, 6, 8 days; but, again, others occurred which lasted 18, 22, 30 days. Although cases of 4 to 8 days' duration occur less frequently, perhaps, under expectant treatment, we do not give the cases lasting so short a time as cures by medicine; for, if we divide the total number of days by the total number of cases of muscular rheumatism, the quotient will be 9 days—exactly the same duration as that obtained by purely expectant treatment.

'We obtain the same result in cases of articular rheumatism. The duration of the cases treated by us was very various. From their commencement till their complete disappearance, some lasted 8 to 11 days; whilst others lasted 14, 20, 30, 40, 50, 60, and even 70 days. Only 7 cases did not last more than 20 days. If we divide the total number of days by that of the cases, we get the average duration of 30 days. So we have exactly the same results as those obtained by the expectant method—the 8 days' duration as exceptional; the 8 to 20 as rare; the 20 to 30 as the usual one; the 30 to 50 days, again, as seldom; and the 50 to 70 days as again exceptional.'[1]

I may here remark that two of Wurmb and Caspar's cases,[2] in which all the symptoms of acute rheumatism had disappeared, with the exception of pain in the joints, and to remove which all the homœopathic remedies had been tried in vain, were immediately cured by the use of vapour baths. This fact is suggestive, and should excite some misgivings in the minds of those who believe in the special power of homœopathic remedies to remove diseased states.

It is to be regretted that hitherto no trustworthy reports of cases of acute rheumatism, treated hydropathically, have been

[1] *Op. cit.* p. 226 et seq. [2] *Op. cit.* p. 242.

published. I have no doubt that hydropathy, when judiciously employed, would exercise a decidedly beneficial influence on that morbid state in a large proportion of cases.

It cannot be doubted that the large doses of active remedies used by many physicians in the treatment of acute rheumatism must produce in many cases most injurious effects, even for years afterwards, on the health of the patients. Every physician knows the injurious and often slowly removed effects of large hæmorrhages on the system; and were even a healthy person half-poisoned with veratrine, his nervous system and digestive organs would probably long feel the effects of it.

To conclude what I have to say on this subject: It would appear that, in proportion as the treatment of acute articular rheumatism is less active, the less risk will there be of complications, and the sooner will the patient's health be completely restored; and that the homœopathic treatment of that disease gives nearly the same results as the expectant method, or properly applied hygienic and dietetic means, with the occasional use of palliatives.

CHAPTER III.

I.—NON-HOMŒOPATHIC TREATMENT OF INTERMITTENT FEVER.

OF all the drugs of the Materia Medica, there is not one whose curative action is so marked and well-established as that of quinine on intermittent fever. I have seen many cases of quotidian and tertian fevers which had been left to themselves for three, six, or twelve weeks, in some of which the paroxysms continued to recur regularly, whilst in others they disappeared and recurred at irregular intervals, completely cured after using the medicine for a few days. However, in the more malignant forms of intermittent fever, even as it appears in Europe, the use of quinine is not always successful. In such cases arsenic has sometimes a more beneficial effect than quinine; but even in malignant forms of the disease, quinine generally gives more satisfactory results than arsenic.

Some medical men suppose that 60 or 70 per cent. of cases of intermittent fever would get well without treatment. When we consider the nature of many of the remedies, popular as well as professional, that have been celebrated for their efficacy in curing ague, the statement appears to be sufficiently probable, at least as far as regards ordinary cases, and when the patients are placed in favourable hygienic and dietetic conditions. In many, however, I believe the progress of the disease towards recovery would be tedious.

There is not much difference of opinion amongst medical men respecting the power of sulphate of quinine, when properly employed, to cure completely the ordinary intermittents of Europe, and of its great efficacy even in cases in which the complaint assumes a malignant or pernicious form. It would be useless to quote authorities to confirm what I have stated; but I may briefly give the opinions of two or three physicians (taken indiscriminately) who have had considerable experience in treating intermittent fever. Dr. Fuchs, Professor of Clinical Medicine at Göttingen, says: '℈ss. of quinine given in the intervals between the paroxysms of quotidian or tertian fevers, almost invariably prevents the return of the paroxysm, of which there is at most only a slight threatening; a second dose is seldom required. In quartan intermittent, on the contrary, the patient must take two doses of ℈ii. when the febrile paroxysm is not present; but in this form of the disease, the effects of the quinine are not so certain as in the former ones.'[1] Dr. Gouzée, of the Military Hospital of Antwerp, when speaking of intermittent diseases, says in such morbid states the sulphas quiniæ 'fait des prodiges; c'est le remède heroique par excellence, le remède universel. Mais il ne faut pas se dissimuler, que son emploi exige toujours beaucoup de prudence.'[2] 'In the majority of the cases, the sulphas quiniæ employed in doses of 5 to 10 grains, dissolved in 1 to 2 ounces of water, with a few drops of diluted sulphuric acid, administered two to three hours before the paroxysm came on, was frequently sufficient to prevent the occurrence of any more. In more obstinate cases, the dose required to be repeated once or twice to produce the desired effect.'[3]

II.—HOMŒOPATHIC TREATMENT OF INTERMITTENT FEVER.

Let us now see what homœopathic physicians say of the treatment of intermittent fever. The only considerable number of cases, of which we have some details given that enable us to judge more or less for ourselves of the results of the treatment, are those published by the able and candid Dr. Wurmb, and his assistant Dr. Caspar, in their 'clinico-homœopathic investigations.' Drs. Wurmb and Caspar received during the year 1850, 110 cases of intermittent fever, in the homœopathic section of the Leopoldstadt Hospital. From these they deducted 33 cases, of which 10 were seized with cholera, 11 left the

[1] Schmidt's *Jahrbücher* for 1857, Band 96, p. 133.

[2] In one of the vols. of the *Archives de Médecine Belge* for 1844, vol. xv. p. 3.

[3] *Bericht des Krankenhauses Wieden von Jahr* 1859, p. 26.

hospital uncured, 4 were in a cachectic state, 3 were cases of relapse, other 3, cases in which no paroxysm had been observed, and 2 were treated with massive doses of quinine; so that, strictly speaking, only 77 cases of intermittent fever could be said to have been treated by them.

Of the eleven cases which left the hospital uncured, one had 10, another 12, a third 13, and two 16 paroxysms. Drs. Wurmb and Caspar remark on this point: 'As six or eight paroxysms are likely to occur under any kind of treatment, it is evident that most of these patients left the hospital for very different reasons than the duration of the disease.'[1]

These physicians considered they made rapid cures, when not more than seven paroxysms occurred after the commencement of the treatment. One of their patients had 26 paroxysms, a second 25, and a third 21, before the disease was cured. They say: 'We homœopathists have every reason to congratulate ourselves on the result of our treatment of intermittent fever.'[2] And again: 'But we must also assert that homœopathy can cure intermittent fevers more surely than any other mode of treatment.' 'From these figures it is evident that, with respect to rapidity of cure, we may most satisfactorily enter the lists with our rivals.'

I need not say that a case of intermittent fever, in which six or seven paroxysms occurred before it got well, when treated according to the method of the old school, would not be regarded as a rapid cure; or that, with proper allopathic treatment, such a prolongation of the disease as to admit of the occurrence of 26, 25, and 21 paroxysms, is not witnessed at the present day under ordinary circumstances. As has been already stated, two or three doses of six or ten grains of quinine are generally sufficient to check the return of the paroxysm.

Drs. Wurmb and Caspar add, however, that in speaking[3] of the cure of intermittent fever, they mean the totality of the disease, and not the more or less rapid removal of one or more symptoms. This is not the same as curing the diseased process. 'Although we cannot prevent the return of a paroxysm with the same certainty that the allopaths can do, yet we have no reason to envy them their remedy, especially as the rapid

[1] *Op. cit.* p. 141. [2] *Op. cit.* p. 150. [3] *Op. cit.* p. 144.

disappearance of the paroxysm is not seldom a very injurious improvement, which deceives both the patient and the physician; and the disease is allowed quietly to progress, only to reappear afterwards with greater violence, and often in a form that is beyond the reach of art. In other cases, in which this does not occur, a cachectic state, which is sometimes incurable, not unfrequently follows the use of the quinine.'[1]

It will readily be admitted by medical practitioners who have had some experience in the treatment of intermittent fever, that Drs. Wurmb and Caspar's account of 'what not seldom takes place' in the allopathic treatment of that disease is very exaggerated and erroneous. It is well known, that in proportion to the frequency of the return of the paroxysms of that disease, the greater is the risk of the supervention of other diseased states—such as enlargement of the liver or spleen, fever cachexy, dropsy, etc. We should be much more likely, therefore, to observe such complications under a system of treatment in which cases with twenty-five and twenty-six paroxysms occur, than under a proper allopathic one, which would probably have reduced their number to two or three.

It seems surprising that Drs. Wurmb and Caspar, who profess to attach little importance to the disappearance of the paroxysm, or, as they term it, to the removal of a symptom, should have administered large doses of quinine to two patients ill of intermittent fever, for the very purpose of removing the paroxysm. The cholera had broken out in Vienna, and had made its appearance in their hospital, selecting its victims chiefly among those suffering from intermittent and typhus fevers. They dared not quietly wait, as they said, for the cure of the intermittent by homœopathic means, but were compelled to suppress the next paroxysm by the use of massive doses of quinine, and thus effect the speedy discharge of the two patients from the hospital. In these cases, they considered the suppression of the paroxysm an *indicatio vitalis*.[2] With notions such as Drs. Wurmb and Caspar held respecting the suppression of the paroxysm, it would be difficult, however, to find a good reason, even in the *indicatio vitalis*, for retaining two patients exposed, as they thought,

[1] *Op. cit.* p. 149. [2] *Op. cit.* p. 143.

during one or two days to the risk of an attack of cholera, in order merely to suppress the paroxysm, and then to send them to their homes with the fever uncured, and the patients consequently as much predisposed to the epidemic in their own houses as in the hospital.

In another part of their essay,[1] the authors say homœopathists have not at their disposal, like the allopathists, a remedy which can so certainly prevent a return of the paroxysm; but preventing a return of the paroxysm is not curing the disease. The homœopathists, instead of suppressing the paroxysm, cure it. They may not be able to do so as rapidly as the allopathists; for the remedies suited to the different states of the intermittent fever do not lie before their eyes, but must often be long searched for, until the proper remedy be found. This statement, as well as the numerous mistakes (77) which they made in selecting remedies for the treatment of 77 cases of intermittent fever, strongly confirm what I said in a former part of my essay about the difficulties attending the practical application of the homœopathic principle.

Ten years afterwards, a physician attached to the same hospital as Drs. Wurmb and Caspar, speaking of a case of tertian fever, in which three paroxysms appeared before the patient entered the hospital, and twenty-six after his admission, before the disease was cured, says: 'The treatment was commenced immediately after the reception of the patient, and six different drugs were tried during its continuance.' The author, Dr. Eidherr, remarks: 'This last case proves how difficult it generally is for us to find the most suitable remedy for a disease, in order to cure it rapidly and permanently. Although this occurs in the majority of cases of disease, it does so particularly in those of intermittent fever. We must admit that it is this form of disease which requires to be very correctly individualized, and of which the treatment most frequently disappoints our expectations.'[2]

If physicians like Dr. Wurmb, who was certainly one of the most distinguished homœopathic practitioners of his day, and Dr. Caspar, who at that time was considered to be a medical

[1] *Op. cit.* p. 145.

[2] *Zeitschrift des Vereins d. hom. Arzc*, vol. i. No. 2, p. 115.

man of experience and ability, found such difficulties in selecting the proper remedies out of fifteen drugs, for a disease so well known to them as intermittent fever, what must be the difficulties of those who, with less skill and experience, have to select remedies for diseases with which they are less familiar! If, instead of selecting their remedies from amongst fifteen drugs, Drs. Wurmb and Caspar had sought for them amongst the sixty remedies indicated by Bönninghausen for the disease, they would probably have made a still greater number of mistakes than they did. Besides, if the views of these physicians respecting the usual method of selecting remedies for intermittent fever be correct, those who follow the old plan must unavoidably commit a greater number of mistakes than we hear of.

I shall now conclude my remarks on the opinions of Drs. Wurmb and Caspar regarding the homœopathic treatment of intermittent fever, by giving an additional extract from their paper on that subject.[1] 'There can be no mistake about the aversion that exists to discuss the subject of intermittent fever in our literature, and that a good many homœopathists are dissatisfied with the results of homœopathic treatment.'

To confirm the truth of this statement, I shall cite the expressed opinions of several eminent homœopathic practitioners. Gross[2] confesses he soon saw that homœopathic drugs were unable to cure intermittent fevers, and he had consequently recourse to quinine in doses of one or two grains. In a public meeting of homœopathic practitioners at Naumburg, Rummel[3] admitted 'that, up to the present time, homœopathy had not discovered the proper remedy for the intermitting element, or for intermittent fever.' Hauptmann[4] confessed that 'he was obliged to have recourse to the secret use of quinine' in the treatment of intermittent fever. Ægidi,[5] in the case of his own son, a boy of four years old, suffering from intermittent fever, could not find a proper remedy in the

[1] *Op. cit.* p. 145.
[2] *Archiv f. d. hom. Heilkunst*, vol. vii. part iii. p. 46.
[3] *Op. cit.* vol. xi. part i. p. 60.
[4] Hartlaub und Trinks, *Annalen h. Klinik*, vol. iv. p. 428.
[5] *Archiv f. d. hom. Heilkunst*, vol. viii. part iii. p. 59.

homœopathic Materia Medica. At last, after fruitless trials, when the boy had got the fourteenth paroxysm, he abandoned the homœopathic treatment, and gave him allopathic doses of quinine.

At a meeting of German homœopathic physicians held at Hanover in the year 1860,[1] Dr. Goldmann stated that, in the part of the country where he resided (Posen), and where intermittent fever was endemic, and often very malignant, homœopathic doses of cinchona and quinine were of no use; whilst larger and stronger ones made rapid cures. On the same occasion Dr. Bushmann praised the use of quinine, even in ℈i. doses, in intermittent fever. Dr. Hirschel[2] recommends the use of strong doses of quinine; and Dr. Weihe of Herford uses large non-homœopathic doses of arsenic in the same complaint.

From what has been said on the subject of intermittent fever, there can be no doubt that the usual allopathic treatment of that disease is far more successful than the homœopathic; and, as we have seen, this fact, or rather the insufficiency of the homœopathic treatment in such cases, has been repeatedly admitted by homœopathists themselves.

As we do not possess a sufficient number of satisfactorily detailed cases of that disease treated expectantly, we cannot appreciate very precisely the value of homœopathic treatment in that complaint. I believe, however, the expectant treatment would give results somewhat similar to those of homœopathy. At all events, the satisfactory results said to have been obtained by the most opposite methods of treatment, lead us to suppose that in many of the reported cures Nature herself did much more than the drugs employed. With regard to the injurious effects frequently produced by the allopathic treatment of that disease, according to Drs. Wurmb and Caspar, every experienced physician will regard them as possibilities that seldom or never occur in cases treated by non-homœopathic physicians of the present day.

[1] *Monatschrift f. Homœop.*

[2] *Hom. Arzneischatz*, Dresden 1856, p. 13.

CHAPTER IV.

I.—NON-HOMŒOPATHIC TREATMENT OF TYPHUS.

THE next disease of the treatment of which I shall compare the results obtained by homœopathic and by non-homœopathic practitioners is typhus fever. In this country a distinction is generally made between typhus and typhoid, or enteric fever. On the Continent, where typhoid fever is of far more frequent occurrence than typhus, physicians generally consider them as merely different forms of the same morbid process. In the Austrian medical schools they are regarded as essentially the same disease. In France, enteric fever is commonly known as typhoid; but there are many French physicians who consider typhus and typhoid fever as only different forms of the same morbid state. Although the morbid lesions found after death seem to draw a marked line of distinction between the two diseased states, it must be confessed that in some cases it is impossible in the living subject to distinguish one form from the other; and it is known that in certain epidemics both may appear in the same locality. To prevent any erroneous conclusions that might be drawn from comparing results furnished by the treatment of different forms of the disease, I shall take care to point out in what sense the term typhus is used in the cases under consideration.

Every medical man acquainted with the literature of his profession, will readily agree with me, that if the treatment of typhus fever were discussed in any medical society, it would be merely a repetition (as far as regards divergence of opinions and absence of fixed principles) of what took place at the French Academy, when the treatment of acute rheumatism was discussed.[1]

[1] I gladly admit, however, that of late years, particularly in this country, a great and satisfactory change has taken place in the treatment of typhus and typhoid fevers (leading to something like uniformity of plan), by limiting the use of drugs, and by trusting more to the influence of dietetic and hygienic means. The writings of Dr. Graves of Dublin, and of Dr. Bennett of Edinburgh, have contributed greatly to this result.

Although this statement scarcely requires to be confirmed, I shall quote the opinions of two or three physicians whose attention has been particularly turned to this subject. In Schmidt's *Jahrbücher*, Heusinger says: 'No physician can put together the various opinions on the treatment of typhus without blushing, since they embrace the whole Materia Medica: purgatives, sudorifics, emetics, tonics, relaxants, stimulants, bleeding, alum, quinine, chloride of sodium, iron, iodide of potassium, calomel, camphor, musk, nitrate of soda, muriatic and sulphuric acids, Seidlitz water, etc.'

In the same periodical,[1] Dr. Millies of Leipzig published an elaborate analysis of forty-seven publications on typhus, in the shape of monographs, essays, etc. At the end of his article he makes the following remarks: 'If we consider, on the one hand, the great variety of the methods of treatment recommended, and the praises that almost every one gives more or less to his own; and on the other, the fact that every method has its deaths, complications, and subsequent diseases, the conviction is forced upon us that typhus is a disease which goes through its cycle of changes uncontrolled by the treatment, and that its course is much more under the influence of other circumstances, partially known to us, than under that of the treatment employed.' In a critical analysis of the principal works published on typhus from 1858 to 1863,[2] Dr. Förster of Dresden says: 'We still find the greatest variety and the most opposite views entertained by practitioners respecting the treatment of typhus, as in many other diseases.'

It would be impossible to make a satisfactory comparison of different methods of treating a disease like typhus, which presents such variety in its forms, and of which the mortality varies so much during different epidemics (from 5 per cent. to 50 per cent.), unless the cases treated occurred about the same time, and in the same place. It is only in the hospitals of Vienna, where these conditions can be realized on a considerable scale, that we must seek for the best comparative data that can be got for the elucidation of the question. I shall briefly mention, however, some results (taken indiscriminately) obtained at

[1] Schmidt's *Jahrbücher*, vol. xcvi. part v. p. 356.
[2] *Op. cit.* for 1863, vol. cxvii. p. 89.

various periods in different parts of Europe by non-homœopathic practitioners, not so much for the purpose of making a formal comparison between them and those of homœopathy, as to show that, if the former are often much less satisfactory than the latter, the latter are sometimes less so than the former.

Dr. Griessinger,[1] who makes a distinction between typhus and ileo-typhus, estimates the average mortality of typhus at 15 per cent. Dr. Smoler,[2] whose observations were made at the Medical Clinique of Prague, where the treatment was chiefly symptomatic, and who, like most of the physicians of the Austrian medical school, considers typhus and ileo-typhus as the same morbid process, gives 15 per cent. as the average mortality, without distinction of age or sex. Dr. Huss of Stockholm,[3] who makes no distinction between typhus and enteric fever, gives 11·5 per cent. as the average mortality, during a period of twelve years, of males, and 8·6 per cent. of females, or 10·6 per cent. of both sexes included. In a report of the medical section of the Civil Hospital of Prague, 34 cases of typhus (including ileo-typhus) are referred to, of which 3, or strictly speaking, only 2, died (the third being carried off by cholera), which gives a mortality of 1 in 17, or about 6 per cent.

The mortality[4] of 447 typhus cases treated in the Edinburgh Infirmary during the year 1865 amounted to 16·33 per cent.; and during five successive years from October 1860, to 15·68 per cent.

If we deduct 16 cases brought to the Infirmary in a hopeless state, and which died within forty-eight hours after admission, the mortality for 1865 would be only 13·23. The number of moribund cases is not given for the other years. At the same hospital, the mortality of 77 cases of ileo-typhus or enteric fever was 6·49 per cent. during the year 1865 ; and in 398 cases, which occurred during five successive years, counting from 1860, it was 10·80 per cent. If we add together the cases of typhus and enteric fever for 1865, deducting the 16 moribund, the mortality will be 12·2 per cent. ; if we add them together for five successive years,

[1] Quoted in Smoler's paper.

[2] *Wiener Zeitschrift* for 1862, vol. xviii. part iii. p. 151.

[3] *Statistics and Treatment of Typhus and Typhoid Fever*, translated from the Swedish by Aberg, London 1855, pp. 45, 41.

[4] Appendix to Report regarding the Royal Infirmary of Edinburgh, 1865.

the mortality will be 12·89 per cent. I may here remark, that at the period to which I refer very little medicine was employed in the treatment of that disease in the Edinburgh Infirmary. More importance was attached to the use of hygienic and dietetic means. Many of the patients were received at an advanced period of the disease, and in a very unfavourable condition.

In his work on Fevers,[1] Dr. Murchison—who, like most British practitioners, considers typhus a distinct form of disease from typhoid or enteric fever—gives the average mortality of typhus in the London Fever Hospital during fourteen and a half successive years (excluding 172, who died within forty-eight hours after their reception) at 17·94 per cent. Of 9485 cases of typhus admitted into the Glasgow Infirmary from 1843 to 1853, 18 per cent. died; and of 1402 cases received during five successive years from 1857 to 1861, 16·83 per cent. died. The average mortality in cases of typhoid or enteric fever admitted into the London Fever Hospital, deducting those which were moribund on admission, as given by Dr. Murchison, was 17·2 per cent. In the Glasgow Infirmary, during twelve years from 1847 to 1853, and from 1857 to 1861, the average mortality of typhoid fever was 18·3 per cent.

According to the report[2] of the large general hospital of Vienna, the Allgemeine Krankenhaus, the mortality of typhus during the year 1861 was 20·4 per cent. In the typhus epidemics of 1852–53 it was 19·2 per cent.; of 1855–56, 20·4 per cent.; of 1856–57, 23·4 per cent; of 1858–59, 19·5 per cent.[3] In the Krankenhaus auf der Wieden of the same city, the mortality in 1859 was 15·7 per cent.; in 1861, 17·22 per cent.; in 1863 and 1864, 22·2 per cent.

Dr. Peacock[4] gives an account of 119 cases of typhus fever—73 males and 46 females—treated during nine months of the year 1855 in St. Thomas's Hospital, London. The treatment was almost expectant—soda water, chlorate of potash, ammonia, infusion of serpentaria, decoction of cinchona. Stimulants and support were given, according to the amount of prostration.

[1] *Treatise on Continued Fevers*, pp. 217, 528.
[2] *Jahrsbericht des Allgemein: Krankenhauses Wien für* 1861.
[3] *Jahrsbericht des Krankenhauses auf der Wieden für* 1859.
[4] *Medical Times and Gazette*, vol. xii. p. 32.

Mean age of males, varying from 4 to 72 years, was 24·5 years; of females, varying from 5 to 58 years, was 24·4 years. Mean period of admission for males was 10·4 days; for females, 9·5 days, or 10·2 days for both. Mortality in males, 13·6 per cent.; in females, 10·8 per cent.: average of both, 12·2 per cent.

Before proceeding to examine the results of homœopathy, I shall briefly advert to those obtained by hydropathy in the treatment of typhus. Although the resources of that method have scarcely been tried in this disease, and although the statements which I am about to quote are by no means satisfactorily established, I confess I entertain the belief that, if it were judiciously applied, along with hygienic and dietetic means, it could render essential service in the treatment of typhus as well as of typhoid fevers. Drs. Brand and von der Decken regard hydropathy, when properly used, as a specific in the treatment of typhus.[1] Dr. Brand says, according to his experience up to the present time, all his cases progressed favourably (although many of them were placed in very unfavourable circumstances), when they were treated from the commencement of the illness; and that many cases were cured, when recourse was had to hydropathy, only in despair of being benefited by any other means. A French physician,[2] M. Leroy (de Bethune), likewise gave a very favourable report of the success of hydropathy in the treatment of typhoid fever during several successive years. M. Leroy bled his patients at the beginning of the disease, in order to prevent the occurrence of congestion. The patient was afterwards enveloped in damp sheets, which were frequently wetted. Cold water was used as a drink.

In 1848,	61	were treated,	of which	2	died.
1849,	22	,,	,,	0	,,
1850,	16	,,	,,	3	,,
1851,	27	,,	,,	1	,,
	126	,,	,,	6	,, Mortality, 1 in 21, or 4·7 per cent.

I have no doubt that a number of M. Leroy's cases, as well as of those of Dr. von der Decken, were not cases of typhus or

[1] Schmidt's *Jahrbücher*, vol. cxvii. p. 124 (1863). They make no distinction between typhus and typhoid fever.

[2] *Union Medicale*, Nos. for Oct. and Nov. 1852.

typhoid fever; at all events, it must have been impossible to make a satisfactory diagnosis of the disease in those cases in which it was said to have been cut short.[1]

II.—HOMŒOPATHIC TREATMENT OF TYPHUS.

We shall now examine what homœopathy has done in the treatment of typhus fever. The only monograph on this subject by a homœopathic physician with which I am acquainted, is one by Dr. Rapou, entitled *Sur la Fievre Typhoide, et son Traitement Homœopathique.* He informs us that, of 70 to 80 cases, some of which came under his treatment at an advanced period of the disease, not one died. There were cases of every degree and variety amongst the patients. Many seemed beyond the resources of art, and left no hope to the physician. In criticising this work, a homœopathic physician, Dr. Gabalda,[2] says: 'His success has led him to advance a proposition which we find too absolute. According to him, typhoid fever, taken in time, and treated homœopathically, presents no danger.' 'On such a delicate subject (the success of the treatment) we prefer to turn to another proposition of the author's.'

But wonderful cures have likewise been made by non-homœopathic practitioners, to some of which we shall refer as counterparts of Dr. Rapou's. Dr. Jacques, quoted by Dr. Eigenbrodt in his brochure, to which I have already referred, states in the *Bulletin de la Société de Medecine de Besançon* for

[1] In the number of the *Lancet* for Sept. 25, 1869, the results of the hydropathic treatment of a large number of cases of typhoid fever are given. They confirm my opinion of the great utility of that method, at least in typhoid fevers:—

Treated by	No. of Cases treated.	No. of Deaths.	Percentage of Cure.
Brand, . .	187	4	2·1
Metzler, . .	78	4	5·
Göden, . .	24	2	8·2
Rätjen, . .	39	0	0·
Jurgensen, .	160	5	3·1
Liebermeister, .	359	33	9·8
	847	48	5·7

The average mortality of these cases is nearly the same as that of Leroy.

[2] *Journ. de la Société Med. Homœop. de France* for 1851, p. 232.

1846, that the mortality of his cases of typhoid fever was $6\frac{22}{313}$ per cent. Another French physician, Dr. Fontan, communicated in October 1865 to the Medical Society of Bordeaux a plan of treating that disease which he had employed for eighteen years, and which was so successful that he did not lose one patient in 184. Professor Sauer, of Pesth, fancies he has found in iodide of potassium a specific against abdominal typhus or typhoid fever. In an article on its treatment, he says, with that medicine, employed before the tenth day of the disease, he saved 97 per cent. of his patients. He usually gave, every hour or two, a tablespoonful of a 3 oz. solution, containing 6 to 10 grs. of iodide of potassium.

No experienced medical man will admit that the successful results recorded by these four authors were obtained in cases of the disease as we usually see it in hospitals, or even in private practice.

During the year 1850, Drs. Wurmb and Caspar—who, like most physicians of the Austrian medical school, make no distinction between typhus and enteric fever—treated 89 cases, of which 11 died, giving a mortality of 12·36 per cent. But we must not overlook the circumstance that Dr. Eigenbrodt, who attended the clinique of Dr. Wurmb during several months of the year 1850, states in his brochure[1] that many of Dr. Wurmb's cases of typhus were so slight, that in allopathic hospitals they would have been classed under the head of intestinal catarrh or gastric fever; indeed, Drs. Wurmb and Caspar seem, by their remarks, to admit the truth of Dr. Eigenbrodt's statement.

They say:[2] 'Should it be objected that we have brought together mild and severe cases of the disease, we would reply, that difference in degree does not constitute an essential difference. Pneumonia is pneumonia, whether both lungs or only one lobe be affected; and small-pox is small-pox, whether few or thousands of pustules appear. We shall not dispute that, in consequence of our views on this subject, we may have committed many errors in our diagnosis. In what relates to the diagnosis of typhus there is little unanimity amongst prac-

[1] *Ueber die Resultate der öffentlichen homœopathische Heilanstalt in der Leopoldstadt zu Wien*, von Dr. Eigenbrodt, Giessen 1854.

[2] *Homœop. Klin. Studien*, p. 83.

titioners,—one calling the disease typhus only when it is so advanced and so decidedly developed, that even an unprofessional person could recognise it; whilst another calls every case of intestinal catarrh typhus.' No one can refuse to admit the truth of Dr. Wurmb's opinion, that mere difference of degree does not constitute an essential difference in disease. I would only remark, that we cannot fairly compare the results obtained from treating diseases so classified with those obtained in the treatment of cases to which the same name was given, but from which many of the lighter ones of the former group were excluded.

To show that some of Drs. Wurmb and Caspar's cases of fever were very mild, I shall relate one of them exactly as it is given in their work, which contains several others similar to the one which I now give:[1]—'Michel Wagner, aged 42. During last nine days complains of debility, loss of appetite, giddiness, confusion of head, sleeplessness, paroxysms of fever, which return every evening; cold, with subsequent heat of long duration, and profuse perspiration. Last night, had considerable delirium.

'Present state: Temperature of body increased, skin moist, face flushed, tongue dry and red; breathing quickened, pulse 88; a little meteoric distention of abdomen, spleen enlarged; no stool for five days; urine scanty—turbid; on chest several small red points, which disappear on pressure; complains of giddiness, noise in the ears, heaviness and heat of head, pressing pain in temples, dryness of mouth and throat, bitter clammy taste, no appetite, great thirst, pain on pressing abdomen; feeling of great weakness, sleeplessness, general discomfort from confused dreams on falling asleep. Rhus ordered. In three days meteorism had almost entirely disappeared; very weak, but feels generally better. From this time the patient rapidly improved, and in fourteen days left the hospital quite well, with the exception of a little weakness.'

Again, the unusually large proportion of eighty-nine cases of typhus fever to thirty-nine of gastro-intestinal catarrh, given in the tabulated hospital report for 1850 by the same physicians, shows still more clearly that their classification of these two diseases was peculiar. In the General Hospital of Vienna, during the years 1850, 1852, 1853, 2742 cases of

[1] *Op. cit.* p. 99.

typhus fever and 3207 of gastro-intestinal catarrh were received, or about $1\frac{1}{6}$ cases of the latter to 1 of the former disease. In the homœopathic section of the Leopoldstadt Hospital, from 1850 to 1852 inclusively, 314 cases of typhus and 141 of gastro-intestinal catarrh were received, or about $2\frac{1}{4}$ cases of the former to 1 of the latter; in the Gumpendorf Hospital, during a period of twenty years, from 1835 to 1855, 3165 cases of typhus and 1181 of gastric fever (as gastro-intestinal catarrh was then called), or about $2\frac{2}{3}$ cases of typhus to 1 of gastro-intestinal catarrh,—a proportion similar to that of Drs. Wurmb and Caspar.

The only year in which I can give the number of cases of typhus and gastro-intestinal catarrh, received in the two large general hospitals, and in the two homœopathic hospitals of Vienna, is 1861:—

	Wiedner Hospital.	Allgemeine Hospital.	Gumpendorf Hospital.	Leopoldstadt Hospital.
Gastro-intestinal catarrh,	737	1749	108	34
Typhus, . . .	544	1389	94	106
Proportion of gastro-intestinal catarrh to 1 of typhus, .	$1\frac{1}{3}$	$1\frac{1}{4}$	$1\frac{1}{8}$	$\frac{1}{3}$

In the year 1861 the proportion of cases of gastro-intestinal catarrh and typhus received in the allopathic hospitals remained nearly the same as it had been for a number of years previously; but in the Gumpendorf Hospital, Fleischmann's manner of arranging these diseases seems to have become similar to that adopted in the allopathic hospitals: in the homœopathic section of the Leopoldstadt Hospital the proportion remained almost the same as it had been in 1850. In short, there can be no doubt that a large number of Drs. Wurmb and Caspar's typhus cases would have been classed among those of gastro-intestinal catarrh in the allopathic hospitals.

I would call the attention of my readers to the comparative number of cases of gastro-intestinal catarrh and typhus fever admitted into the Gumpendorf Hospital and the allopathic and homœopathic sections of the Leopoldstadt Hospital, during a series of six or seven successive years, as it is chiefly from the results furnished by them that the comparative merit of the homœopathic and non-homœopathic treatment of typhus must be determined. As will be seen, the proportion of cases of gastro-intestinal catarrh to those of typhus fever is much larger in the allopathic section than in the other two. It is evident we cannot fairly compare the results of treatment of two groups of cases, when in one of them there are some so mild that they would be excluded from the other. I think I shall be able, however, when comparing the results of treatment in these two groups, to adopt a plan that will neutralize any errors arising from diagnosis, or from difference of views in the manner of grouping them.

During seven successive years, from 1859 inclusive, 791 cases of gastro-intestinal catarrh and 706 of typhus fever were received in the Gumpendorf Hospital, giving a proportion of about 1·12 of the former to 1 of the latter. At the same period, in the homœopathic section of the Leopoldstadt Hospital, during six successive years, 609 cases of gastro-intestinal catarrh and 483 of typhus were received, giving a proportion of about 1·25 cases of the former to 1 of the latter disease; and at the same time, during six years, 786 cases of gastro-intestinal catarrh and 421 of typhus were received in the allopathic section of the same hospital, giving a proportion of about 1·85 cases of gastro-intestinal catarrh to 1 of typhus.

The only other report known to me which contains a large number of cases of typhus treated homœopathically in a hospital, is that furnished by Dr. Fleischmann. In 1855 he published the results obtained in the treatment of 3165 cases of typhus fever, during a period of twenty years, in the Gumpendorf Hospital, of which 368 died, giving an average mortality of 11·62. The value of Fleischmann's tables is much diminished by the complete absence of details respecting them.[1] The number of cases of gastro-intestinal catarrh, or of gastric fever, as he then termed it, amounted to 1181 during the same period; so that, although in the year 1861 the proportion of these cases to those of typhus was not much less than in the large allopathic hospitals of Vienna, in this table it is very different,

[1] At the article 'Pneumonia,' see some remarks on Dr. Fleischmann's diagnostic skill.

—in fact, nearly the same as that given by Wurmb and Caspar in their clinical investigations, or 1 case of gastro-intestinal catarrh to $2\frac{2}{3}$ of typhus. I am inclined to suppose, therefore, that by the year 1861 Dr. Fleischmann's notions on typhus had undergone some change.

I should now compare the results of the homœopathic with those obtained in the allopathic hospitals of Vienna. I must remark, however, that although two of the conditions which I consider essential to a fair comparison of the results of homœopathic and non-homœopathic treatment (that the diseases treated should have occurred about the same time, and in the same place) co-exist in the reports of cases which I possess of the two large allopathic and two homœopathic hospitals of Vienna, yet I doubt if a still more important circumstance—the similarity of the cases, as far as regards their general severity—can be said to co-exist. For reasons formerly assigned, there can be no doubt that a number of Drs. Wurmb and Caspar's cases of typhus, as well as of those contained in Dr. Fleischmann's report from 1835 to 1855, would not have found their counterparts amongst those of the Wiedner and Allgemeine Hospitals. It has been remarked by physicians who visited the homœopathic hospitals at Gumpendorf and the Leopoldstadt, and the two large general hospitals, that the cases seen in the former were less severe than those which presented themselves in the latter. But independently of the greater general severity of the cases treated in the latter, there is another circumstance that exercised, I have no doubt, a considerable but indeterminate influence in increasing the mortality of disease in these establishments. I mean the agglomeration of such a large number of sick (1800 to 2000 in the Allgemeine) in the same locality, the injurious influence of which no amount of attention to cleanliness and to the purity of the air could entirely counteract.[1]

In general, the professional reputation of the allopathic hospital physicians of Vienna stands deservedly very high. Their

[1] The recent investigations of Sir J. Simpson on Hospitalism amply prove the truth of my opinion with regard to surgical hospitals. I have no doubt similar results would be obtained by examining the reports of large medical ones.

treatment of disease for many years past has been chiefly palliative; yet the mortality of certain diseases, at least in the two large allopathic hospitals, has been constantly higher than in some in other parts of Europe,—as in the Edinburgh Infirmary, in which the general plan of treatment has been for a long time past similar to that adopted in the former.

As the great mortality in the two large hospitals of Vienna cannot be ascribed, therefore, either to want of skill on the part of their physicians, or to the injurious activity of their treatment, it must be owing to other causes; and we should certainly be led into error, were we to compare the results obtained in them, with those of the homœopathic hospitals of the same city. Fortunately, I shall be able to make a satisfactory comparison between the results obtained during a period of several successive years in the Gumpendorf Hospital, and in the homœopathic section of the Leopoldstadt Hospital, and those obtained in the allopathic section of the same hospital. Both are under the management of the Sisters of Mercy, and organized in the same manner. Similar classes of the population resort to them. In short, the circumstances in which the patients are placed, are as nearly as possible the same in both.

The following data, relative to the mortality of typhus, are copied from the printed annual reports of the Superior of the Sisters of Mercy:—

Year.	Gumpendorf Hospital.		Homœopathic Section of Leopoldstadt Hospital.		Allopathic Section of the Same.	
	No. of Patients received.	No. of Deaths.	No. of Patients received.	No. of Deaths.	No. of Patients received.	No. of Deaths.
1859	183	17	102	14	93	8
1860	78	16	44	6	50	12
1861	92	17	103	6	Report wa	nting.
1862	132	20	102	20	94	19
1863	81	5	60	6	48	5
1864	42	9	Report wa	nting.	61	8
1866	98	17	72	10	75	4
	706	101	483	62	421	56
	Mortality, 14·30 p. ct.		12·83 per cent.		13·30 per cent.	

So that the mortality in the Gumpendorf Hospital is 1 per cent. greater, and that in the homœopathic section of the Leopoldstadt ·47 per cent. less than that of the allopathic section of the same hospital. We may thus consider the results of the allopathic and homœopathic treatment of typhus as nearly the same.

We have already shown that Fleischmann, as well as Wurmb and Caspar, undoubtedly placed a number of cases in their category of typhus, which in the allopathic section of the Leopoldstadt Hospital would have been classed among the cases of gastro-intestinal catarrh. If this opinion be well founded, we may find, on adding together the milder cases of fever, or gastro-intestinal catarrh, as they are now generally named, and those of typhus, that the therapeutic results will not be so favourable to homœopathy as those which we have just given with typhus fever alone.

Year.	Gumpendorf Hospital.		Homœopathic Section of Leopoldstadt Hospital.		Allopathic Section of Same.	
	No. of Cases of Gastro-Intestinal Catarrh treated.	No. of Deaths.	No. of Cases of Gastro-Intestinal Catarrh treated.	No. of Deaths.	No. of Cases of Gastro-Intestinal Catarrh treated.	No. of Deaths.
1859	81	0	133	0	100	0
1860	104	1	87	0	92	0
1861	108	0	90	0	Report wanting.	
1862	141	0	104	0	151	1
1863	100	3	74	0	197	1
1864	112	1	Report wanting.		109	0
1866	145	4	121	1	109	0
	791	9	609	1	758	2
Cases of Typhus,	706	101	483	62	421	56
Total,	1497	110	1092	63	1179	58
	Mortality, 7·34 per cent.		5·76 per cent.		4·91 per cent.	

Or, in other words, the allopathic treatment gives better results by 2·43 per cent. than the homœopathic at the Gumpendorf Hospital, and by ·96 per cent. than the same method at the Leopoldstadt Hospital.

Gastro-intestinal catarrh—or, as it was formerly named, gastric fever—is the only disease that is likely to be mistaken for typhus. In a given number of cases of gastro-intestinal catarrh and typhus, the treatment of the latter might be made to appear more favourable than it really was, by classing some cases of the former as typhus; whilst, at the same time, the results of the treatment of gastro-intestinal catarrh would appear less favourable than they actually were, in proportion to the number of cases of that disease treated as typhus.

I shall here present a tabulated summary of the mortality of homœopathic and non-homœopathic treatment, of which I have given some details in the preceding pages, that the reader may be able to compare them at a glance.

NON-HOMŒOPATHIC TREATMENT.			
HOSPITALS.	Mortality of Typhus Fever.	Mortality of Enteric or Typhoid Fever.	Mortality of Mixed Cases of Typhus and Enteric Fever.
	Per cent.	Per cent.	Per cent.
Prague Hospital, 1850	...	...	6
Stockholm, Dr. Huss	...	...	10·6
London, St. Thomas' Hospital, 1855	12·2	...	...
Edinburgh Infirmary, 1865	13·22	6·49	...
Edinburgh Infirmary, 5 years, from 1859	...	...	12·89
London Fever Hospital, 14½ years	17·94	17·20	...
Glasgow Infirmary, from 1843 to 1861	17·4	18·37	...
Allopathic Section of Leopoldstadt Hospital	...	...	13·30
Vienna, Wiedner Hospital, 1859 to 1861	...	...	19·29
Vienna, Allgemein Hospital, 4 epid., 1852 to 1859	...	...	20·5
Leroy (Hydropathy), 1848 to 1851	...	4·7	...

HOMŒOPATHIC TREATMENT.	
Homœopathic Section of Leopoldstadt Hospital, 1850	12·35
Homœopathic Section of Leopoldstadt Hospital, during 6 years, from 1859 to 1866	12·83
Gumpendorf Hospital, during 20 years, 1835 to 1855	11·62
Gumpendorf Hospital, during 7 years, from 1859	14·30

The average mortality of the mixed cases containing typhus and typhoid fever, but of which I believe the latter formed the larger number, is 13·58 per cent., or, if we include M. Leroy's cases, 12·31 per cent., which is almost the same as that given by the homœopathic hospitals of Vienna. I do not pretend, however, to make a formal comparison between some results obtained by practitioners of the old school taken indiscriminately at different periods and in different places, and those obtained in the homœopathic hospitals of Vienna; but merely to show in a general way, that in many cases the results obtained by the former are nearly the same as those obtained by the latter.

The only satisfactory comparison that could be made was between results obtained in the allopathic section of the Leopoldstadt Hospital, and those obtained in the homœopathic section of the same hospital, and in Fleischmann's at Gumpendorf, which, as we have seen, were all nearly the same. As far as regards typhus and typhoid fever, the results of homœopathic treatment in the hospitals of Vienna are nearly equal to the most successful, and superior to the general, results of non-homœopathic treatment. Can they be ascribed to the action of the homœopathic drugs? I think not. The latest as well as the earlier results of homœopathic treatment in the Vienna hospitals are so similar to those obtained by expectant or palliative treatment in the allopathic section of the Leopoldstadt Hospital, and in others in different parts of Europe, that we are naturally led to ascribe them to some cause or causes common to both systems of treatment. The drugs employed cannot be regarded as such; in short, I think the common causes are—1*st*, that tendency of the diseased organism, to which I have often adverted, to return to a healthy state under certain circumstances; and 2*d*, the judicious use of hygienic and dietetic means.

CHAPTER V.

I.—NON-HOMŒOPATHIC TREATMENT OF CHOLERA.

I SHALL now proceed to examine the results obtained by the non-homœopathic treatment of cholera. When discussing the subjects of acute rheumatism and typhus fever, it was shown that practitioners of the old school could not pretend to have any fixed principles in their treatment of these diseases; and the same may be said with still more truth of their treatment of cholera. Numberless remedies and plans of treatment, said to be based on experience, have been confidently recommended, only to lead to failure when tried by other practitioners. As our experience in treating that disease has increased, our faith in remedies has gradually become less: indeed, it would now be difficult to suggest a medicine or plan of treatment that has not been already tried in vain. Although remedies even the most heroic have ceased to inspire hope, yet, in presence of the intense sufferings of the patients, and of the rapidly fatal progress of the disease, no one, as far as I am aware, has yet ventured to try anything like an expectant plan, although numerous well-authenticated cases are on record (I myself have heard of a good many unpublished ones) in which individuals recovered from severe attacks of cholera without the aid of drugs.

The mortality of that disease under non-homœopathic treatment seems to be so nearly the same in every quarter of the world, at every season of the year, and under every possible variety of treatment, that it would be useless to give any particular reports to illustrate it. It has varied from one-third to two-thirds of those attacked, or from about 30 per cent. to 70 per cent.

II.—HOMŒOPATHIC TREATMENT OF CHOLERA.

Homœopathy owes much to cholera; for it was in consequence of Fleischmann's success in treating that disease, that permission was granted to homœopathic practitioners to treat patients according to their system throughout the Austrian dominions.

If any faith can be given to the numerous published results of the homœopathic treatment of cholera, we must admit that homœopathy has been far more successful than allopathy in the treatment of that disease. I shall be able to show, however, on the testimony even of homœopathic writers, that many of these reports are not trustworthy. I do not mean to accuse their authors of having intentionally falsified facts; but I have no hesitation in saying that, in their zeal to support a new system of medicine on its trial, they have done what has frequently been done by authors of the old school, who had some favourite doctrine to support: they have viewed facts in such a manner as to lead to erroneous representations, and to false interpretations of them.

In a work published in 1863,[1] Dr. Stens, speaking of the more favourable results obtained by the homœopathic treatment of cholera, says: 'Strikingly favourable was this ratio (proportion of cures to deaths) in the treatment of cholera. According to the accurately prepared tables to which I have referred, the mortality of that disease, when treated homœopathically, was only 8½ per cent.; whilst under allopathic treatment it was 51½ per cent., and even 67 per cent. during the last epidemic.'[2]

Of the value of these and similar statistics, I shall give the opinion of one of the editors of the *British Journal of Homœopathy*, vol. xv. p. 130: 'In the sixteenth letter[3] we find the rather rash assertion, that the homœopathic mortality in cholera is only 8½ per cent. Now we should rejoice much were this the case; but, alas, we know from sad experience that it is at least three times as high as here stated.' 'We know very well the data on which the percentage of mortality he gives is founded, and we are well convinced of their utter untrustworthiness.' What would the same critic have said of the statement of Dr. Rubini of Naples, who some time ago published a pamphlet on the treatment of cholera with a saturated solution of camphor in alcohol, in which he boasted that, with the aid of one or two physicians, he had

[1] *Die Therapie unserer Zeit*, p. 116.

[2] *Rosenberg: Fortschritte und Leistungen der Homœopathie.*

[3] Of Dr. Stens' work.

treated 680 cases of the disease, without the loss of a single patient? If we could suppose that they were simply cases of choleraic diarrhœa, or cholerine, we should have had reason to doubt the truth of this statement; but as Dr. Rubini recommends the saturated alcoholic solution of camphor as an infallible remedy in all cases and stages of cholera, we may safely consign his statement without further discussion to the category of therapeutic myths.

In order, however, to show that some homœopathists of the present day no longer blindly accept Hahnemann's opinions, and that they even ignore the great merit formerly ascribed to him of having proclaimed from the provings of camphor, before he had seen a case of cholera, that it was the specific remedy for that disease, I shall quote the remarks of one or two writers on the subject. Dr. Ker says:[1] 'The most successful treatment, however, with camphor during the former visitation of the disease, has never attained a percentage of recoveries over two-thirds of the cases treated. In the provings of camphor, one cannot but be struck with the absence of stomach and bowel symptoms which are such prominent and characteristic ones of the real disease. In the provings of camphor in the *Materia Medica,* under the heading "Stool," involuntary diarrhœa is the prominent symptom: the other symptoms (mostly empirical) scarcely resemble those of cholera. Hahnemann said it must be given during the first two hours from the commencement of the "sickening." If they (the two hours) refer to collapse only, then in that case there has been too much boasting about the infallibility of the camphor cure; for it is a misapplication of terms to call a remedy infallible, that needs not be given except during the first two hours of the disease. If they refer to the preliminary diarrhœa, or merely premonitory signs, then in a scientific point of view camphor fails, and should no longer be quoted amongst cholera remedies.'[2]

In a subsequent number of the same journal, a German homœopathic physician, Dr. Hirsch, confirms Dr. Ker's remarks.

[1] *British Journal of Homœopathy* for 1867, p. 137.

[2] On this point, allopathic and homœopathic practitioners appear to have exchanged their former opinions. See article Camphor in Dr. Ringer's *Handbook of Therapeutics.*

He says : 'Still, in relation to these cholera cures with camphor by Rubini, it is very remarkable that, among the effects of the drug on the healthy subject, we are unable to find the group of symptoms in the sphere of the digestive apparatus that would remind us of the picture of cholera. Considering the rather limited indication for the employment of camphor in cholera, I confess I am astonished to find the medicine recommended by Dr. Rubini as the most certain and infallible remedy for all cases and all stages of cholera. In his pamphlet on the subject we meet with such glaring contradictions, that I am inclined to doubt the infallible virtue of the remedy.'

The general mortality of the homœopathic treatment of cholera, according to the more trustworthy reports, varies from 20 to 50 per cent. I have not seen any reports of the treatment of that disorder in the homœopathic and allopathic hospitals of Vienna during the year 1850, except those given by Drs. Wurmb and Caspar in their work to which I have so frequently referred, and by the latter in his pamphlet entitled *A Parallel between Homœopathy and Allopathy.*

During the cholera epidemic of 1850, 423 cases were received into the Allgemeine or General Hospital of Vienna, of which 227 died, giving a mortality of 53·6 per cent. During the same period, 171 cases were received into the homœopathic section of the Leopoldstadt Hospital: of these 60 died, giving a mortality of 35 per cent. The 171 cases include 15 given apart in the general report, and which occurred before the opening, and after the shutting up, of the separate cholera ward. The difference, then, in favour of homœopathy, according to these data, is not, as Dr. Caspar says, 17·6 per cent., but 18·6 per cent. He adds, however : 'Perhaps a stricter diagnosis in the allopathic than in our hospital has contributed to this great difference.'

Dr. Eigenbrodt[1] and others have remarked the absence of any such term as cholerine or diarrhœa choleraica in the homœopathic hospital reports ; but the same remark may be applied to those of the allopathic hospitals of Vienna and Prague. It is very probable that in the allopathic, as well as in the homœopathic hospitals of Vienna, a number of cases of cholerine or choleraic diarrhœa were received ; but under what name,

[1] *Ueber die Resultate*, p. 30.

whether they were classed with cases of gastro-intestinal catarrh or of cholera, I cannot say. I have reason to believe that the cases of cholera reported from the allopathic hospital were all cases of genuine cholera. I think the cases of cholerine received into the homœopathic hospital were not classed with those of gastro-intestinal catarrh, otherwise Dr. Caspar would have mentioned the circumstance when discussing the diagnosis of cholera. He quotes the opinion of Professor Hammernjck of Prague on that subject, which he adopts: 'When, during the presence of an epidemic of cholera in a place at a certain period, even a small group of symptoms, such as loss of appetite, occasional pains in the bowels, diarrhœa, etc., make their appearance in a number of individuals,—when these show certain peculiarities, that they do not yield to the use of opiates and diet; when, in addition to these, severe casual symptoms at times occur—that after their disappearance the urine becomes albuminous; when their appearance cannot be well accounted for otherwise, and which at other periods do not present themselves in this manner; then they should be considered not as precursory symptoms, but as the disease itself—cholera. It would be a great mistake to designate them otherwise, as cholerine, cholera gastrica,' etc.

There can be little doubt, then, that Drs. Wurmb and Caspar placed under the name of cholera cases of a similar nature, but of a much lighter form, and which elsewhere would have been reported as cholerine or diarrhœa choleraica. I do not object to their views on the subject; I would only remark, that we cannot fairly compare the results of their treatment of cholera with those of practitioners who excluded such cases from their reports of the disease.

Dr. Caspar says:[1] 'Although we are convinced that in the General Hospital likewise, not a small number of milder cases were treated, we will, however, suppose the contrary, and deduct from our total the sufficiently marked but milder cases, and to the classification of which, therefore, objections might be made. That would leave 137 cases with 60 deaths, or a mortality of 43·7 per cent.; still 9·9 per cent. less than in the Allgemeine Hospital.'

I may here remark that, for reasons given at page 160, I do

[1] *Op. cit.* p. 46.

not think we can fairly compare the results of the homœopathic with those of the large General Hospital of Vienna. The manner in which cholera cases were classed in the homœopathic section of the Leopoldstadt Hospital would of itself render the comparison unsatisfactory.

In his pamphlet, to which I have just referred, Dr. Caspar says: 'In the second medical division of the General Hospital of Prague, in the year 1849, 217 cases of cholera were received, of which 48 per cent. died, leaving a difference of 13 per cent. in favour of homœopathy. This disproportion can be partly, but only to a small extent, accounted for by the circumstance that 20 per cent. of the patients of the Prague Hospital, while only 13 per cent. of ours, were above the age of 50. To have proportionally so many cases above the age of 50 as in the Prague Hospital, we require to add 12 to our number. Supposing 84 per cent. of these 12 cases died, as in the Prague Hospital, our mortality would be increased 11 per cent., and we should require to give more than 41·5 per cent., so still 6·5 per cent. less than in the Prague Hospital.'

Dr. Caspar appears to have made one or two mistakes in his calculations. He required to add only 10 per cent. to his mortality, which would make it not 41·5 per cent., but 45 per cent., or 3 per cent. less than in the Prague Hospital. We must not overlook the circumstance that the two groups of cases occurred in different places and at different periods.

In the fourteenth volume of the *British Journal of Homœopathy*, there is a report of the cases of cholera treated at the Gumpendorf Hospital by Dr. Fleischmann during the various epidemics from 1835 to 1855, amounting altogether to 1202, with 409 deaths, or a mortality of 34 per cent. Dr. Fleischmann gives no details whatever respecting the cases. Of the treatment he says: 'In the treatment of the disease, at least as we have it in our hospitals, much remains for us homœopathists to wish for. Every medicine that has been recommended has been tried and tried again by me; but I have little to say in praise of any of them.' The mortality of Dr. Fleischmann's cases is nearly the same as that given by Drs. Wurmb and Caspar for 1850.

Entering a little into the examination of his own cases, and those of the Vienna and Prague Hospitals, Dr. Caspar admitted the influence of causes which induced him to modify considerably the reported amount of the mortality of his cases; and had Dr. Fleischmann acted in the same manner, I have no doubt he likewise would have been obliged to give a different percentage of mortality to the one quoted from his report. Indeed, without at all wishing to impugn his truthfulness, I have no hesitation in saying, that if a physician sufficiently acquainted with the disease, and the condition of the patients treated in the allopathic and homœopathic hospitals of Vienna, had examined them from a non-homœopathic point of view, he would have modified considerably the reported amount of mortality by homœopathic treatment.

The next report of cholera treated homœopathically to which I shall refer is that of M. Tessier,[1] who during the invasion of the epidemic was one of the physicians of the Hospital of Ste. Marguerite of Paris. The number of cases treated by M. Tessier amounted nearly to 100, with a mortality of 48 to 49 per cent.; whilst that of the patients treated in the non-homœopathic section of the same hospital was 59 to 60 per cent. There could be no doubt about the correctness of the diagnosis of the cases treated.

It had been agreed upon, in order to avoid subsequent recrimination, that two patients should be placed alternately in each section as they arrived. In this way, as near an approach, as was practicable, was made to general equality in the condition of the patients in the two sections. Dr. Valleix, another physician of the same hospital, stated in a periodical of the day, that during the first half of the epidemic the worst cases had been sent to the non-homœopathic section; but I think Dr. Timbart has satisfactorily shown that the statement was quite unfounded. The only circumstance to be regretted is the small number of the cases treated. So far as they go, they gave 10 to 11 per cent. in favour of homœopathic treatment. It must be confessed, however, that a mortality of nearly 50 per cent. is not a satisfactory result for a system of treatment that claims to be κατ' ἐξοχὴν specific. It is, in short, only 10 to 11 per cent. superior to treatment that has no fixed principles, and of which some of the remedies were decidedly injurious.

[1] *Recherches cliniques sur la Pneumonie et le Cholera*, Paris 1850.

Is this superiority in the result due to the action of homœopathic medicines? I do not believe so, and some homœopathic practitioners have expressed a similar opinion. At page 139 of the *British Journal of Homœopathy* for January 1867, the author of an article on cholera says: 'I believe our greater success to be owing to our interfering less with nature's own processes, and to our admirable dietetic and hygienic appliances.' At the present day, I believe it is generally admitted that many of the plans of treatment formerly employed by physicians of the old school against cholera increased more or less the natural mortality of the disease; and, in fact, a striking picture of the treatment of the last epidemic in Europe was the general rejection of heroic methods, and a more general recurrence to the use of stimulants of one kind or another.

Long after the preceding portion of this work was written, I was able to procure a copy of the part of the printed report for 1866 of the establishments in the Austrian Empire placed under the care of the Sisters of Mercy, which contains one of the cases of cholera treated in the Homœopathic Hospital at Gumpendorf, and in the homœopathic and allopathic sections of the Leopoldstadt Hospital. The patients treated in them were as nearly as possible in the same general condition, and the results of the treatment consequently deserve serious attention.

Cases of Cholera treated at Vienna during the Epidemic of 1866.

In the Gumpendorf Hospital.		Homœopathic Section of the Leopoldstadt Hospital.		Allopathic Section of the same Hospital.	
No. Treated.	No. of Deaths.	No. Treated.	No. of Deaths.	No. Treated.	No. of Deaths.
248	67, or 27 p. ct.	17	6, or 35·29 p. ct.	242	58, or 23·96 p. ct.

The mortality in the Gumpendorf Hospital was 3 per cent., and in the homœopathic section of the Leopoldstadt Hospital, about 11 per cent. greater, than in the non-homœopathic section. The number of cases treated in the homœopathic section of the Leopoldstadt Hospital was so small, however, that we cannot attach great importance to the results obtained there. The fact that so few cholera patients were treated in that section, whilst such a large number were treated in the allopathic one, requires explanation. During the epidemic, the treatment of cholera in the General Hospital of Vienna, where the mortality was 43·4 per cent., as well as in the other allopathic hospitals of that city, was chiefly symptomatic or palliative. I may here remark, that in treating that disease, homœopathic practitioners generally abandon the use of infinitesimal doses. To conclude, the homœopathic and non-homœopathic treatment of cholera, as tried in the same hospitals at the same period, and on similar classes of patients, gives, on the whole, nearly the same results: they were as much in favour of homœopathy at Paris, as they were against it at Vienna. As far as regards the action of drugs, neither plan seems to have any notable influence on the disease.

CHAPTER VI.

I.—NON-HOMŒOPATHIC TREATMENT OF PNEUMONIA.

THE last disease to the treatment of which by homœopathic and non-homœopathic practitioners I shall direct attention, is pneumonia. For a long time previous to the middle of the present century, there was considerable uniformity in the views of the physicians of the old school respecting its treatment. As it was generally regarded as the type of inflammatory diseases, and at the same time admitted to be one of a very formidable nature, the most active antiphlogistic treatment was directed against it. The free use of the lancet, the administration of mercurials and antimonials, and a strict regimen, formed the essential part of it. During that period the mortality varied from 1 in 3 to 1 in 6 cases of the disease.

There can be no doubt whatever, that the great changes which have taken place more recently in the treatment of this disease, must be ascribed in the first place to the results obtained in practice by some homœopathic practitioners, especially by Dr. Fleischmann of Vienna. They emboldened Dr. Dietl to make trial of the expectant plan on a gigantic scale in this disease; and the result of his trial has been to upset the firmly grounded belief in the indispensable necessity for using venesection and other heroic remedies in the treatment of pneumonia. Dr. Hughes Bennett, guided by his pathological views on the subject, has assailed the part of the antiphlogistic treatment which Dr. Dietl left unchanged, by proposing, besides the abandonment of venesection and drugs, a complete change in the regimen so generally recommended in that disease.[1] What remains, then, of the antiphlogistic method in the treatment of pneumonia, or even of inflammation in general—the only part of the therapeutics of

[1] The results of his practice, which extended over a period of fifteen years, appear to have sadly puzzled his critics.

the old school that had assumed something like a systematic form ?

But opinions held for centuries are not abandoned in a day, even by the most advanced thinkers; and at present there is a large body of practitioners who still employ the strict antiphlogistic method in the treatment of pneumonia. But as they have before their eyes the results obtained by homœopathic treatment of that disease (whatever they may think of its efficacy), and those obtained by the expectant method, as well as by the restorative plan recommended by Dr. Hughes Bennett, it is impossible to doubt that they will ultimately modify greatly their treatment of it, and even, perhaps, abandon entirely their former notions on the subject.

Although pneumonia does not appear to be under epidemic influence to such a marked degree as several other diseases, still it would be desirable, in judging of the comparative value of homœopathic and non-homœopathic treatment, to select for comparison cases treated about the same period, and in the same locality. I shall, however, give some results of hospital treatment in different places, and at different periods, which will be of greater value the longer the period over which the observations extend.

During the earlier part of the present century, the mortality of pneumonia varied from 15 per cent. to 35 per cent.[1] M. Louis of Paris published the result of his treatment of 107 uncomplicated, favourable cases, subjected to active antiphlogistic treatment, of which 32 died, so that the average mortality was about 30 per cent.[2] When the bleeding was performed during the first four days of the disease, the mortality was less than a half of what it was when performed during the first nine. M. Grisolle recommended a more moderate use of the lancet. The average mortality of his 232 uncomplicated cases was a little less than 16 per cent. When bleeding was performed during the first period of the disease, the mortality was 1 in 10; when it was performed in the second stage, 1 in $5\frac{3}{4}$.

Professor Rasori of Milan treated with large doses of tartrate of antimony 648 cases of pneumonia, of which 143 died, or

[1] *Recherches sur les Effets de la Saignée*, Paris 1835.

[2] *Traité pratique de la Pneumonie*, Paris 1841.

about 22 per cent. Grisolle treated with the same medicine 154 cases, of which 143 died, or about 18·8 per cent.; and Dietl 106, of which 22 died, or about 20·7 per cent.

Dr. Lebert of Zurich modified still more than Dr. Grisolle had done, the antiphlogistic treatment of pneumonia, by diminishing the amount of depletion in that disease. Of 205 cases treated by him in the hospital of that town during a period of five successive years, 15 died,[1] giving an average mortality of 7·3 per cent.; of complicated cases, 4 died; of 201 uncomplicated, 11 died, or a little more than 1 in 18. Among the total number of cases, there were 24 of double pneumonia. Bleeding was not employed when there was considerable prostration of strength; and if it occurred at a later period of the disease, Dr. Lebert had recourse to cordials and nourishment.

One of the most important works on the treatment of pneumonia is that published by Dr Huss[2] of Stockholm in the year 1861. During a period of sixteen successive years he treated 2616 cases of that disease, of which 281, or 10·74 per cent., died. The number of uncomplicated cases was 1657, of which 96 died, or about 1 in 17¼ cases; of complicated cases, 959, of which 185 died, or a little more than 1 in 5·15. There were 384 cases of double pneumonia. During the former half of the sixteen years the antiphlogistic treatment was carried out with considerable vigour, with a mortality of 11·54 per cent.; during the latter half of that period, general bleeding was not employed, and local, only in exceptional cases, with a mortality of 10·21 per cent. There was thus a difference of 1·33 per cent. in favour of the latter period.

In the year 1852 Dr. Kissel[3] published a very elaborate historical sketch of the treatment of pneumonia. He likewise gave a detailed account of cases which he himself had treated (probably in private practice) with unusual success. During an epidemic of that disease he treated at Oberlahnstein, near the Rhine, 32 cases of pneumonia (his patients consisted of children as well as adults) by means of a solution of corrosive sublimate,

[1] Deducting 17 cases which died on the day of their reception or on the following day.

[2] *Die Behandlung der Lungenentzündung*, Leipzig 1861.

[3] *Die directe Kunstheilung der Pneumonien*, Eilenburg 1852.

½ grain—1 grain in 6 ounces of water. It is not said in what doses it was given. No deaths occurred. In most cases the solution caused purging, but no salivation; neither general nor local bleeding was employed. The treatment relieved the symptoms, but it did not control the natural course of the disease; so Dr. Kissel did not consider corrosive sublimate a direct curative remedy. He considers as directly curative only those drugs which accelerate the natural progress of the disease, or diminish its duration. He regards as such the tincture of the acetate of iron, prepared according to Rademacher's formula, and the tincture of the acetate of copper.

Of the former he gave 1½ drs. daily, and treated 19 patients with it from December 1847 till September 1848, of which 1, a child of six months old, died, giving a mortality of 5·2 per cent. From the autumn of 1848 till the summer of 1850 he treated 93 cases with the tinct. acet. cupri in doses of ℥i. daily, of which 4, or 4·3 per cent., died. The average mortality of the 112 cases was 4·4 per cent. The average duration of the disease, from the first day of fever till complete convalescence, was 7·7 days when treated with iron, 7·5 days when treated with copper. Average duration of treatment, commenced from first to third day of disease, was 4 days; when from fourth to eighth day, it was 4·25 days. Of the uncomplicated cases, none died. Of complicated cases treated with iron, 1 died; of those treated with copper, 4 died—2 from meningitis, 1 from marasmus, and 1 from tubercles.

Dr. Kissel appears to have examined his patients with great care; and although it is much to be regretted that he did not publish the histories of some, at least, of his cases, the manner in which his book is written leaves no doubt that he was a physician quite competent to form a correct diagnosis of the disease. We must not, however, overlook two circumstances: 1*st*, that, with the exception of 4, his cases were uncomplicated; and 2*d*, that a large proportion of them (70) were under the age of twenty. Now, as I shall afterwards have occasion to show, these two circumstances have a great and favourable influence on the treatment of pneumonia.

I am surprised, however, that his success has not induced other medical men to repeat his trials, if we except perhaps some made by Professor Sauer of Pesth, and one or two other practitioners, with the sulphate of copper. Some account of Professor Sauer's observations was published in vol. cxiii. p. 357 of Schmidt's *Jahrbücher* for 1862. Of 56 cases, 3 died, giving a mortality of 5·4 per cent. The medicine employed, in which Professor Sauer fancied he had found a specific, was the sulph. cupri grs. vi., and aq. ℥vi.: a tablespoonful of the solution, to which a little opium was added, was given every one or two hours. In the work referred to no account is given of the cases treated.

At page 149 of his work to which we have already referred, Dr. Kissel gives a short account of the hydropathic treatment of about 40 cases of pneumonia by Dr. Hegelé, head physician of the Würzburg Hospital, during the years 1848 and 1849, without any deaths. The patients, principally young and robust individuals, generally felt much relieved on the second day of the treatment. In the majority of the cases a critical sweat occurred on the second, less frequently on the third day. Unfortunately, no further details are given of the treatment. I may here remark that Dr. Armisted, who has published a work on hydropathy, considers that method not at all adapted to the treatment of pneumonia.

I shall next notice the results of a plan of treating pneumonia recommended by Dr. Hughes Bennett of Edinburgh, and which he calls the restorative. It resembles the expectant method in this respect, that it employs drugs very sparingly, and bleeding seldom, if ever; but it differs from it in a very important point —the amount of nourishment which it allows to the patient at every period of the disease. 'At the commencement of the treatment,' Dr. Bennett says, 'I order as much beef-tea as can be taken; and as soon as the pulse becomes soft, nutrients, and from ℥iv. to ℥viii. of wine daily.'[1] During his pathological researches on this disease, he found that, before resolution can take place, the effused matter must be transformed into pus cells, which are soon broken up and absorbed. Pus cells, says Dr. Bennett, must be regarded as living growths, and as such require an excess of blood, good nutrition, and exalted vital force, to hurry on their development, and carry them through

[1] *Principles and Practice of Medicine*, by J. H. Bennett, 5th edit. p. 696.

the natural stage of their existence. With these views, he was naturally led to try the restorative treatment, which he now recommends, and of which the result has been remarkably favourable. Of 129 consecutive cases publicly treated according to that plan by Dr. Bennett in the Royal Infirmary of Edinburgh, up to the year 1865, 4 only died from severe complications, or 1 in 32¼, giving a mortality of 3·1 per cent. This result is very remarkable, especially when we reflect that about one-third of the patients had their health previously impaired; and we must not overlook the circumstance that Dr. Bennett's observations extend over a period of sixteen years. Although his opinion that, as pus cells are living bodies, they require an excess of blood, etc., is a theoretical one, the results of his method of treating pneumonia in adults are undoubtedly the most successful with which I am acquainted. It strongly recommends itself, therefore, to medical men as worthy of trial; and I have no doubt that in a few years it will have been sufficiently tested to enable them to pronounce a decided opinion on its value.

We shall now examine what has been accomplished by employing the so-called expectant method in the treatment of pneumonia. This I consider the most interesting part of our clinical investigation, not only in reference to homœopathy, but likewise in reference to the general therapeutics of the old school. It was the investigation of this point that led most directly, on the Continent at least, to the important changes that have subsequently taken place in the general treatment of disease. As I have formerly said, it was the results obtained by homœopathic practitioners, particularly by Dr. Fleischmann of Vienna, that suggested to Dr. Dietl the idea of employing the expectant method in the treatment of that disease; and it must be confessed that the conclusions at which he arrived, and which he published in a work entitled *Der Aderlass in der Lungenentzündung*,[1] took the medical world by surprise. That is not to be wondered at, when we consider that in the treatment of 189 cases of a disease, until that time regarded as a most formidable one, he laid aside the classic antiphlogistic plan, and with the exception of a few palliative remedies, left the disease to follow its natural course; more especially when we consider the results

[1] Wien, 1849.

that he obtained by what he called dietetic treatment—namely, a mortality of only 7·4 per cent., or 1 death in 13½ cases. I should mention that all the fatal cases were complicated.

The trial to which we have just referred extended over a period of three years, 1844, 1845, 1846. In the fifth, sixth, and seventh numbers of the *Wiener Medicinische Wochenschrift* for 1852, he published the results of a similar but more extensive trial from the year 1847 to 1850. Of 750 cases, 69 died, giving a mortality of 9·2 per cent., or 1 in 10·87. The condition of the large majority of these cases was by no means favourable to the trial of the expectant method. Only 274 of the patients were of robust habit of body; the rest were either weak or of but moderate strength: 134 cases only had previously enjoyed good health; 616 had suffered from various diseases, of which 514 were severe, and had occurred shortly before the attack of pneumonia, leaving their bad effects still manifest in the organism. Of the 514 cases of severe disease, 246 were cases in which the respiratory organs had been affected, and of which almost all had been bled: 319 of the cases were complicated. The average duration of the disease was twenty days.

Except in some complicated cases, his treatment was purely expectant. He frequently employed such palliative remedies as mixtura oleosa, potio acidula, infusum liquiritiæ, mixtura gummosa, etc., which could not exercise any marked influence on the progress of the disease. In his work on bleeding, Dr. Dietl says: 'Pneumonia runs its course best when not interfered with by medicines;' but he does not mean to say that all treatment is superfluous, or injurious. In many cases, he says, venesection is an excellent symptomatic means; and in cases of complication with severe bronchitis, in which suffocation is imminent, cupping and counter-irritation may save the patient.

The unexpected results of the trial do not appear to have provoked much criticism, or to have excited much doubt of their trustworthiness, although he alludes in his account of the second trial to a 'vote of want of confidence.' From what he says, it appears he had been blamed for not having published the histories of his cases. 'In dem, ich mit Beruhigung über dieses Misstrauensvotum hinausgehe, weise ich auf die colossale Aufgabe 380 (189 would have been enough for the first series) Krankengeschichten zu drucken, und auf die weit noch colossalere sie zu lesen hin, erbiethe mich hingegen jedem Leselustigen die sämmtlichen Krankengeschichten im Manuscripte, mitzutheilen.' I may quote another and rather remarkable statement from the same report: 'Even when blood is not abstracted, treatment is often useful, even indispensable' ('Die Behandlung häufig nützlich, ja unentbehrlich ist'). At the time of the trials Dr. Dietl was head physician of the large general hospital 'auf der Wieden' in Vienna, and was recognised as a skilful practitioner and accurate observer; and the sectional physicians under him were quite competent to diagnose pneumonia, and to observe the influence of the dietetic treatment on its progress.

There is one circumstance, however, which, in the absence of any printed documents containing the histories of the cases, throws doubt on the correctness of Dr. Dietl's statements. It is the fact that, in the same hospital in which he had made his observations, the mortality of pneumonia had enormously increased a year or two after he had left it for a chair in the University of Cracow. According to Dr. Dinstl's account, published in the year 1854, it was about 20 per cent.; and in printed reports which I have of that hospital for the years 1859, 1861, 1863, 1864, it was respectively 25 per cent., 20·33 per cent., 20·86 per cent., 27·23 per cent. What was the cause of a mortality two or three times greater than in Dr. Dietl's time? There was no change in the general condition of the patients treated after Dr. Meltzer became head physician of the hospital. Bleeding and drugs were perhaps more frequently employed in the treatment of pneumonia than when Dr. Dietl held that appointment; but certainly not to such an extent as to have any marked influence on the mortality of the disease. Did Dr. Dietl select his cases? It does not appear that he did so. I have heard it suggested that the great number of complicated cases was the cause of the subsequent increased mortality; but the proportion of complicated cases of pneumonia diminished rather than increased after Dr. Dietl's departure. The proportion of complicated to uncomplicated cases in the 750 which he had treated was 52 to 100, or rather more than one-half; in the same hospital in the year 1859, it was 50·4; in 1861, about 42·8; in 1863, 43·3; and in 1864, 44·6, to 100.

Under these circumstances, I think Dr. Dietl lies under a grave obligation to medical science: he must either publish his cases in a tabulated form, or he must explain the cause of the great difference in the mortality of pneumonia in the Wiedner Hospital during the two periods to which I have referred. If the cases could be published with proper details, they would constitute most valuable illustrations of the natural course of the disease. If a number of the histories of the cases have been too briefly taken to be satisfactory medical documents, would it not be better frankly to state so? We know that the diagnosis of a disease may be correctly made, and its progress carefully watched, although its history may be very incompletely written. I think Dr. Dietl's reason for not publishing the histories of 380, or rather of 189 cases, treated expectantly, 'that it would be a colossal undertaking,' cannot be considered satisfactory by medical men.

In the year 1861, Dr. Eidherr, of the homœopathic section of the Leopoldstadt Hospital of Vienna, published the histories of 106 cases of pneumonia (although not with all the desirable details) in a journal that has not a large circulation. It could not, therefore, be considered a very formidable undertaking for Dr. Dietl, who had a large medical staff at his service, to have published *in a tabular form* an account of the 189 cases, at least, of the first series treated expectantly. In the meantime, Dr. Dietl's results can only be accepted conditionally, although their accuracy, as far as regards cases between six and fifteen years of age, has been indirectly confirmed by other observers.

In a posthumous paper by Legendre on croupal pneumonia,[1] or, as he terms it, *pneumonie franche*, the following statement is made: 'When genuine pneumonia occurs accidentally in a person in good health, it terminates habitually, not to say always, favourably, at least in children. Catarrhal pneumonia is a more formidable complaint.'

From 1854 to 1861, Dr. Barthez[2] observed (in hospital practice) 212 cases of genuine pneumonia in infants (or rather

[1] *Archives gener. de Médecine* for 1859, p. 205.

[2] *Med. Times and Gazette*, May 1, 1862.

in children from two to fifteen years of age), and lost only two patients from double pneumonia, or about 0·94 per cent. of his cases. The one-half of these children were not subjected to any treatment; in a number of them it was limited to an occasional purgative, emetic, or bath; and in one-sixth only of the cases were active remedial means employed. Antiphlogistic treatment was altogether rejected. *If no treatment was employed, resolution commenced on the sixth to the eighth day, more especially on the seventh. In some cases it began as early as the third or fourth day.*

Dr. Ziemssen[1] says: 'According to my observation, primary croupal pneumonia in children terminates almost always in complete recovery, when the patient has been previously strong and healthy, placed in favourable external circumstances, and not subject to depressing treatment.'

Of 201 cases, 7 died, and 4 terminated in other complaints—tubercles and dilatation of the bronchi, of which 2 died. Of 201 cases, then, 190 were completely cured. The mortality was 3·48 per cent.; or, including the 2 who died of diseases brought on by pneumonia, 4·47 per cent.

In the 201 cases blood was abstracted eleven times—twice by venesection, four times by cupping, and five times by leeches. In about half the cases scarcely any treatment was employed; a little weak lemonade, etc., were given, chiefly for the appearance of doing something.

II.—HOMŒOPATHIC TREATMENT OF PNEUMONIA.

We shall now turn our attention to what homœopathy has accomplished in the treatment of this disease. The cases reported by Dr. Fleischmann naturally present themselves first to our attention; for it was these cases which, in this country at least, first seriously drew the attention of medical men to homœopathy. The results obtained by Dr. Fleischmann were so contrary to what the generally received opinions in therapeutics would have led practitioners to expect, that very few of them were disposed to admit their accuracy. As pneumonia was one of the most frequent and successfully treated complaints

[1] *Pleuritis und Pneumonie im Kinderalter*, Berlin 1862, p. 256.

in Dr. Fleischmann's hospital reports, and as it received special notice from Dr. Balfour in his famous report on Fleischmann's hospital practice, discussion was naturally turned more particularly to that disease, at least in this country. Some erroneous statements were made both by those who attacked and by those who defended Dr. Fleischmann's statistics; but unfortunately the discussion, instead of being conducted in a calm and dispassionate manner, assumed on more than one occasion the miserable appearance of personal dispute, with which medical science had but little connection.

The principal objection made to the accuracy of Dr. Fleischmann's statistics of pneumonia, was the comparatively small number of cases of bronchitis given in his report. It was justly remarked by his opponents, that bronchitis is a disease of much more frequent occurrence than pneumonia. 'How comes it, then,' said they, 'that in Dr. Fleischmann's reports there is such a large number of cases of the latter, and such a small one of the former disease? The cases must either have been selected, or the diagnosis must have been frequently erroneous.' It was stated that bronchitis was a disease of comparatively rare occurrence in Vienna. As far as regards the term bronchitis, the statement is true, even at the present day; but the diseased state which in this country has been designated by that term, was at that period, and still is, of frequent occurrence.[1] Formerly it was called febris catarrhalis, and sometimes, but rarely, Lungenröhrentzündung, or bronchitis; now it is generally called Lungencatarrh. Misapprehension respecting the meaning of the word contributed not a little to prolong and obscure the discussion. I believe the supposition with regard to the selection of the cases was quite unfounded; for, with few exceptions, the patients are received both at the Gumpendorf and Leopoldstadt Hospitals by the Sisters of Mercy, who act as apothecaries, without having been previously seen by the physician.[2]

[1] In the Austrian medical school the term bronchitis has not been much used; and those most closely corresponding to it—febris catarrhalis and Lungencatarrh—appear not to have been used with great precision. The latter term includes all inflammatory affections of the mucous membrane of the lungs, and has a more extensive application than bronchitis.

[2] The sister who has charge of the pharmacy, and who admits the patients into the hospital, may have as much knowledge of disease as an

The question of diagnosis is a more delicate one. There is an essential difference, however, between an erroneous statement and a falsehood; and without wishing to impugn in the slightest degree the veracity of Dr. Fleischmann, I will frankly state my conviction on the subject.

It was not till about the year 1840 that the medical school of Vienna began to give much attention to physical diagnosis. Dr. Fleischmann had finished his studies long before that period. In 1835 he was appointed physician to the Gumpendorf Hospital. It is doubtful if he knew much of auscultation at the time of his appointment; and it is well known that, without special instruction on the subject, a long time must elapse before a medical man can make himself familiar with its practice, even with the advantage of an hospital for learning it.[1] I shall mention one or two facts which seem to justify the opinion of his colleague, to which I have referred in the note.

In the large allopathic hospitals of Vienna at that time, as well as subsequently, and in the allopathic section of the Leopoldstadt Hospital, the proportion of cases of febris catarrhalis (including those of bronchitis)—or, as it is now generally termed, Lungencatarrh—to those of pneumonia has varied from $1\frac{1}{4}$–5 (generally from $1\frac{1}{2}$–2) of febris catarrhalis to 1 of pneumonia. In Fleischmann's reports from the year 1835 to 1848, the proportion of cases of febris catarrhalis to those of pneumonia is very different; the number of cases of pneumonia received during a year being always greater than those of febris catarrhalis and bronchitis taken together, as the following table of the number

experienced hospital nurse. In general, chronic or organic diseases, especially if the patient's health appears to be much impaired by them, are not received into the hospitals of Gumpendorf or of Leopoldstadt, as it is said a greater amount of good can be done to the poor by admitting acute cases only which do not require such a long residence in the hospital. Patients with organic diseases, however, are not unfrequently admitted, probably from the sister being unable to detect them.

[1] One of the editors of the *British Journal of Homœopathy* says, in a note to p. 473 of the fifteenth volume of that periodical (1857), 'that hitherto even his bitterest opponents admit him (Dr. Fleischmann) to be a proficient in physical diagnosis.' I have already mentioned that one of his homœopathic brethren who knew him well did not estimate his skill in diagnosis so very highly.

of cases received, and of the years in which they occurred, will show :—

Years when treated, ...	1835	1838	1839	1845	1847	1848
Bronchitis : number received, .	—	1	—	2	6	3
Febris catarrhalis, do. .	2	3	1	15	47	38
Pneumonia, do. .	16	19	26	47	80	46

In the Gumpendorf Hospital, during a period of seven successive years commencing from 1859, the average proportion was $2\frac{1}{3}$ cases of Lungencatarrh to 1 of pneumonia ; in the homœopathic section of the Leopoldstadt Hospital it was $2\frac{1}{2}$, and in the allopathic section of the same it was $3\frac{7}{8}$ of the former to 1 of the latter disease.

We see, then, that the proportion of cases of bronchitic affections to those of pneumonia has gradually increased in a remarkable manner in Fleischmann's reports, from $\frac{1}{8}$ in 1835, and $\frac{1}{26}$ in 1839, to $2\frac{1}{3}$ subsequently to 1859 ; whilst in the allopathic hospitals of Vienna the proportion during that time did not present any great variation. What was the cause of this enormous difference in the proportion of Fleischmann's bronchitic and pneumonic cases ? Every impartial medical man will admit that, at the commencement of his career in the Gumpendorf Hospital, he must have classed his cases of pulmonary disease in a peculiar manner. Did this arise from his not being so familiar with the diagnosis of pulmonary disease ? In short, did he commit mistakes in diagnosis ? I confess I have no doubts on that point.

In the report which he published in the year 1855 of twenty years' practice, Dr. Fleischmann gives 593 cases under the heading of cough (acute and chronic), of which 17 died. Would any physician have been considered a proficient in the physical diagnosis of diseases of the chest, who nineteen years ago reported such a large number of cases of cough (to say nothing of 50 additional cases under the heading of spasmodic cough), with so many deaths, which he could not refer to any known diseased state of the respiratory organs ?

In the same report to which I have referred, 5 cases of emphysema are given, all of which died,—a rate of mortality so unusual, that the termination of the 5 cases has been written probably by mistake in the column of deaths instead of that of cures. At all events, the reported success of his treatment of that disease in subsequent years would seem to justify that supposition. During the years 1860, 1862, 1863, 1864, Dr. Fleischmann treated 16 cases of emphysema (exclusive of 3 remaining at the end of the last year), of which 1 died, and 15 are reported cured. It has been affirmed, however, even by homœopathic physicians, as Dr. Müller in one of his reports of the former Homœopathic Hospital at Leipzig, and Drs. Wurmb and Caspar in an article on chronic catarrh in their work entitled *Clinico-Homœopathic Studies,* that emphysema is an incurable disease.

These additional facts only tend to confirm the reasonableness of the doubts expressed with regard to Dr. Fleischmann's proficiency in the physical diagnosis of diseases of the chest.

Before terminating this digression, I must point out what I have frequently had occasion to do—the striking inconsistency between the practice of eminent homœopathic practitioners, and the usually received doctrines of homœopathy. Hahnemann and many other writers on homœopathy have insisted on the importance of individualizing disease, of carefully tracing the resemblance between the characteristic symptoms of a case, and those produced by the drug to be employed. But Dr. Fleischmann uses only a single medicine, a minute dose of phosphorus, in all cases of pneumonia, in both sexes, in all ages, and at every stage of the disease; and the result of his practice proves at least the worthlessness of those wearisome searches, considered so indispensable, after an exact simile, for each individual case.

But although Dr. Fleischmann regards phosphorus as a specific for pneumonia, some eminent homœopathic physicians, such as Drs. Wurmb and Caspar, and Dr. W. Arnold, entertained a very different opinion with regard to the curative power of that medicine in pneumonia. In their remarks on the remedies employed in pneumonia, at the end of their article on that disease, Drs. Wurmb and Caspar say: 'If we speak here of phosphorus among the remedies indicated in fibrinous pneumonia, it is not so much because we consider it a ὅμοιον for that disease, as because we wish to excuse ourselves for having used it in cases in which our selection of it was not conformable to the spirit of the homœopathic law. It has a certain and decided action on the respiratory organs. It possesses in a rare degree, as shown by the experiments of Orfila, Magendie, Bibra, and Geist, the property of causing a deposit in the substance of the lungs; but it does not answer to the requirements that a homœopathic remedy for fibrinous inflammation of the lungs should possess. The question arises, not only where it acts, but likewise how it acts; and in this respect there is no agreement between the characteristic symptoms of the drug and those of the disease. The resemblance between them relates to their seat of action, and not to their nature: it exists between individual symptoms or groups of symptoms, but not between the characteristic features of either; it is apparent only, not real. In short, phosphorus produces an alteration of the blood the reverse of the fibrinous, and it cannot therefore possibly be the homœopathic remedy in cases of fibrinous pneumonia. Whilst we refuse to admit any efficacy to phosphorus in fibrinous pneumonia, other homœopathic practitioners prescribe it not only in that, but in all other kinds of pneumonia: some of our colleagues regard it as a sort of universal specific in this complaint. How phosphorus can suit at one time typhus cases, and at another fibrinous pneumonia; how in one case it increases, and in another diminishes, the plasticity of the blood; in short, how one and the same remedy can be the *ὅμοιον* for essentially different diseases, we cannot comprehend.'

The next report of the homœopathic treatment of pneumonia which I shall notice is that furnished by Drs. Wurmb and Caspar in their work to which I have often referred. They treated 19 cases of the disease during the year 1850, all of which recovered. All the cases, except two, were uncomplicated; the one with chlorosis, the other with intermittent fever. Probably neither of these morbid states increased much the gravity of the cases.

Dr. Eigenbrodt[1] has shown very satisfactorily, in his pamphlet on the results of the treatment in the homœopathic section of the Leopoldstadt Hospital, that Drs. Wurmb and Caspar

[1] *Op. cit.* p. 22.

arranged some of their cases of pulmonary disease in a manner different from that generally adopted by medical men. Complicated cases of pneumonia were placed by them under the name of the complication: thus, the cases of four individuals who had suffered more or less from chronic catarrh for some years, but who were received into the hospital to be treated for pneumonia, which had recently occurred, were classed under the head of chronic catarrh. Two of these cases died. It would certainly have been more in accordance with the general custom of medical men, to have designated the cases by the name of the disease for which they were specially treated.

The same physicians reported three cases of pleuro-pneumonia, of which one died. The autopsy of one of them was made, and showed that the more prominent element of the diseased state was pneumonia. With the exception of a small portion of the right upper lobe, the entire pulmonary substance was in a state of grey hepatization. The pleural cavity (we must suppose, then, only one of them) contained a turbid, yellow fluid; the pleuræ were covered with a thick, yellow purulent exudation. In this case I think it will be admitted that pneumonia was the principal disease. So that if this case had been classed as Dietl and others would have done, we should have had 24 cases of pneumonia, with 3 deaths, or a mortality of 12·5 per cent.; or if, with Eigenbrodt, we add the other 2 cases classed as pleuro-pneumonia, we should have 26 cases of pneumonia, with 3 deaths, or a mortality of 11·53 per cent.

Later on, in a small pamphlet published in 1854, Dr. Caspar gives an account of 92 cases of pneumonia, with 6 deaths, or a mortality of 6·5 per cent. He enters into a critical examination of his own cases and the 750 of Dr. Dietl. Taking into consideration the age, sex, etc., of the two groups of cases, he admits that the mortality of his cases would require to be increased to 8·6 per cent., which would give 0·8 per cent. in favour of homœopathic treatment. But Dietl had comparatively fewer complicated cases than Caspar, by making allowance for which there would be ultimately 1·5 per cent. in favour of homœopathy. Dr. Caspar appears to have been anxious to have his list of complications as large as possible: thus, he has 1 case of curvature, 2 of chronic enlargement of the spleen, 2 of decrepitude or old age, although allowance must have been made for these two cases when discussing the subject of the comparative ages of the patients of the two groups.

In the year 1862, Dr. Eidherr, attached to the same hospital as Drs. Wurmb and Caspar, published the histories of 106 cases of pneumonia treated in the hospital from 1849 to 1860, all of which recovered. But they were cases selected to illustrate the action of certain homœopathic potencies. With few exceptions, the whole of one lung, and in some cases both lungs, were affected at the same time.

In the homœopathic section of the Leopoldstadt Hospital, 92 cases of pneumonia were received during the years 1850, 1851, and 1852,—a proportion that would give 306 cases in ten years. As one entire lung was affected in almost all Eidherr's cases, his proportion of extensively developed cases of pneumonia to the supposed total number of cases received, would be unusually large. The average annual mortality among the cases treated in the hospital during nine successive years,[1] beginning from 1850, as given in manuscript by Dr. Eidherr, was 7·2 per cent. I may remark that, with a very few exceptions, the treatment of the 106 cases was commenced from the first to the fifth day of the disease inclusively; and M. Grisolle has shown what a great influence that circumstance has on the result of the treatment. About 45 of Eidherr's patients were between twenty and thirty years of age, and 7 only were above forty-nine. About two-thirds of the patients were females. The cases were uncomplicated, with the occasional exception of pleuritis, which was sometimes very severe. The majority of the patients enjoyed good health previous to the attack of pneumonia. With the exception of the sex, all the circumstances which I have mentioned exercise a marked influence in diminishing the mortality of the disease; indeed, from the observations of Dietl, Barthez, Ziemssen, Bennett, and others, we have seen that the mortality of uncomplicated pneumonia is very small.

As Dr. Eidherr's reported cases occurred from time to time during a period of ten years, and as their number probably amounted to about one-third of the total number of cases of

[1] I exclude the patients of 1859 (the tenth year), as they will be placed in another series of cases.

pneumonia treated during ten years in the homœopathic section of the Leopoldstadt Hospital, we may conclude that, as far as regards the general condition of the patients, their age, sex, and the period of the disease when the treatment was commenced, the remaining two-thirds were similar to those whose histories have been given. In short, making the necessary allowances both for and against the average mortality of his cases, as regards age, sex, etc., we shall find that it is greater than that of some non-homœopathic practitioners.

It is worthy of remark that, in Dr. Eidherr's reported cases, *resolution generally commenced from the fifth to the eighth day of the disease inclusively: in most of the cases it commenced on the sixth or seventh day.* Contrary to what many homœopathic physicians have stated, that soon after the commencement of the treatment the disease is checked, in Eidherr's cases *it continued to progress and extend itself for two, three, or even four days before resolution commenced.*

The next report of cases of pneumonia treated homœopathically, is that of Dr. Tessier of Paris. He treated 41 cases, of which 3 died, giving a mortality of 7·3 per cent., or almost the same as what occurred in Dr. Dietl's first trial of the expectant method. All Tessier's cases, with the exception of 3, were males; but 12 of them were above fifty years of age, and 2 above seventy. In the majority of the cases, *resolution commenced on, or about the seventh day of the disease;* and in this respect his observations harmonize with those of Eidherr, as well as with those of the physicians who have employed the expectant method. This I consider a circumstance of the highest importance, as it would seem to show that homœopathic treatment does not notably influence the progress of the disease. Indeed, Dietl has stated that the results of homœopathic and dietetic treatment are the same.

Fortunately I have been able to procure a copy of the printed reports for several successive years of the hospitals in charge of the Sisters of Mercy, by the superior of the order. I shall here give a copy from them of the results of the treatment of pneumonia in the Gumpendorf, and in the homœopathic and allopathic sections of the Leopoldstadt Hospital. I believe they are the only reports, besides those of Tessier, that will enable us to make something like a satisfactory comparison between the results of the two methods of treatment, on account of the similarity in the general condition of the patients in the two hospitals. The reader must bear in mind, however, what has been said respecting the diagnosis of Dr. Fleischmann, the manner of classing diseases of the chest by Drs. Wurmb and Caspar, as well as the much smaller proportion of cases of Lungencatarrh in the homœopathic than in the allopathic wards.

Years.	Gumpendorf Hospital.		Homœopathic Section of Leopoldstadt Hospital.		Allopathic Section of same Hospital.	
	No. Treated.	No. of Deaths.	No. Treated.	No. of Deaths.	No. Treated.	No. of Deaths.
1859	22	3	16	2	18	2
1860	51	3	10	1	18	3
1861	16	...	12	...	Report wanting.	
1862	25	1	13	1	18	...
1863	42	1	19	2	17	6
1864	50	4	Report wanting.		13	1
1866	33	2	24	3	20	1
	239	14	94	9	104	13
	Mortality, 5·85 p. ct.		Mortality, 9·57 p. ct.		Mortality, 12·5 p. ct.	

This table gives the most successful results to the homœopathic treatment. The average mortality in the Gumpendorf Hospital was 3·72 per cent. less than in the homœopathic section of the Leopoldstadt, and 6·65 per cent. less than in the allopathic section of the same hospital. In the homœopathic section of the Leopoldstadt Hospital it was less by 2·93 than in the allopathic section.

It is much to be regretted that no details have been given of the cases treated in these hospitals. I have no doubt, however, that if Dr. Fleischmann had given us some respecting his cases, as Drs. Wurmb and Caspar have done in their work to which we have so often referred, impartial criticism would have considerably modified his reported results. I have already shown that he must have committed a number of mistakes in classing his cases of pulmonary catarrh and pneumonia, but which I cannot attempt to estimate from want of details. We can only accept conditionally, therefore, the results of the treatment of pneumonia by Drs. Fleischmann, Wurmb, and Caspar.

From what has been said respecting the classification of cases of pulmonary catarrh in the homœopathic hospitals, it will be interesting to know the reported results of the allopathic and homœopathic treatment of that disease in the above-mentioned hospitals. I shall give it without details. During seven successive years, 554 cases of Lungencatarrh were treated in the Gumpendorf Hospital, of which four died, giving a mortality of 0·72 per cent.; in the homœopathic section of the Leopoldstadt Hospital, 238 cases, with four deaths, or a mortality of 1·68 per cent.; in the allopathic section, 402 were treated, of which five died, giving a mortality of 1·24. The results, then, of the treatment of Lungencatarrh are less favourable to homœopathy than those furnished by the treatment of pneumonia. The mortality of the Gumpendorf Hospital is less by ·52 per cent. than that of the allopathic section; but the mortality of this section is less than that of the homœopathic by ·44 per cent.

From the data which I gave when examining the treatment of pneumonia, there can be no doubt that some of the results of non-homœopathic treatment, particularly when few or no drugs were employed, have been more satisfactory than the most successful furnished by homœopathy. I think we may also conclude from the same data, that the less pneumonic patients were bled or drugged, the greater was the success of the treatment. I should add, however, that the results which I have given of homœopathic treatment are superior to those observed under non-homœopathic treatment, as it is usually carried out.

I must here remark that Dietl's statement, that 'homœopathic and dietetic treatment give nearly the same results,' appears to be confirmed by the fact, that both in those patients who used few or no drugs, and in those who were treated homœopathically (Tessier's and Eidherr's), resolution commenced about the same period—from the fifth to the eighth day of the disease. The occurrence of resolution at this period has been so frequently remarked, that I consider it a part of the natural course of the disease under ordinary circumstances; and it seems to prove that homœopathic treatment does not accelerate it, or, in other words, does not shorten the duration of the complaint.

In order that the reader may be able to see at a glance some of the principal results of the homœopathic and non-homœopathic treatment of pneumonia, under various conditions, I shall arrange them in a tabular form under these two heads. I shall divide the cases treated by physicians of the old school into three groups, corresponding to the activity of the treatment employed, although the division is open to objections. 1*st*, That in which no bleeding and few or no drugs were employed; 2*d*, That in which medicine was more employed, and bleeding sparingly; 3*d*, That in which both medicine and bleeding were more or less actively employed.

	Where or by whom Treated.	No. of Patients Treated.	No. of Deaths.	Average Mortality.
				Per Cent.
	Homœopathic Treatment—			
	Fleischmann (7 successive years), . .	239	14	5·85
	Eidherr (7 successive years), . . .	...	...	7·22
	Homœopathic section of Leopoldstadt Hospital (6 successive years), . .	94	9	9·57
	Do., Wurmb and Caspar, 1850, . .	24	3	12·55
	Tessier, ...	41	3	7·3
	Non-Homœopathic—			
1st Group.	Hegelé, Hydropathic Treatment, . .	40	...	...
	Barthez, Expectant do. . .	212	2	0·94
	Ziemssen, do. do. . .	201	7	3·48
	Dietl, 1st series, do. do. . .	189	14	7·4
	Do., 2d series, do. do. . .	750	69	9·2
2d Group.	Bennett, Restorative do. . .	129	4	3·1
	Kissel, with Acet. Ferri or Acet. Cupri, .	112	5	4·4
	Sauer, with Sulph. Cupri, . . .	56	3	5·35
	Edinburgh Infirmary (1865), . .	36	3	8·33
	Allopathic section of Leopoldstadt Hospital (6 successive years), . .	104	13	12·50
	Gen. Hosp. of Vienna (10 suc. years), .	...	...	18·28
	Huss (8 successive years), 2d series, .	...	...	10·21
3d Group.	Lebert, ...	...	...	7·3
	Huss (8 successive years), 1st series, .	...	...	11·50
	Grisolle, ...	...	...	16
	Louis, ...	...	...	30

CHAPTER VII.

GENERAL MORTALITY IN SOME HOMŒOPATHIC AND NON-HOMŒOPATHIC HOSPITALS—SUMMARY OF THE COMPARATIVE RESULTS OF THE TREATMENT OF PARTICULAR DISEASES.

I SHALL conclude my examination of the comparison of homœopathic and non-homœopathic treatment by giving two comparative tables of the annual general mortality in some hospitals at Paris and Vienna. I shall first notice one published by M. Tessier, giving the annual amount of mortality in the homœopathic and non-homœopathic sections of the Hospital of Ste. Marguerite at Paris, during the years 1849, 1850, and 1851:—

Year.	Homœopathic Section.		
	No. of Persons Treated.	No. of Deaths.	Mortality.
1849	1292 males and females.	126	9·75 per cent.
1850	1677 ,,	138	8·22 ,,
1851	1694 ,,	135	7·96 ,,

So 4663 patients, with 399 deaths, which gives a mortality of 8·55.

Year.	Non-Homœopathic Treatment.		
	No. of Persons Treated.	No. of Deaths.	Mortality.
1849	1087 males and females.	169	14·71 per cent.
1850	1195 ,,	107	8·99 ,,
1851	1442 ,,	135	9·36 ,,

Total number of patients treated, 5724, of which 411 died, or a mortality of 11·3 per cent., which gives 2·75 per cent. in favour of homœopathic treatment.

The only other hospital reports between which I can make a comparison are those of the Gumpendorf Hospital and of the homœopathic and allopathic sections of the Leopoldstadt Hospital: the first during a series of seven years; the latter two, during one of six years:—

Year.	Allopathic Section.		Homœopathic Section.		Gumpendorf Hospital.	
	No. of Patients Treated.	No. of Deaths.	No. of Patients Treated.	No. of Deaths.	No. of Patients Treated.	No. of Deaths.
1859	590	34	758	29	971	71
1860	553	34	605	16	997	71
1861	Report wanting.		816	17	775	14
1862	805	39	856	34	1015	68
1863	821	30	830	23	984	57
1864	718	28	Report wanting.		1034	50
1866	673	19	704	29	962	60
Total,	4160	184	4569	148	6738	391
	Mortality, 4·4 per cent.		Mortality, 3·2 per ct.		Mortality, 5·8 per ct.	

The mortality, then, in the allopathic section is 1·4 per cent. less than in the Gumpendorf Hospital, and 1·2 per cent. higher than in the homœopathic section. It is remarkable that the general mortality in Fleischmann's hospital is almost double that of the homœopathic section of the Leopoldstadt Hospital; although, in the particular diseases of which I have had occasion to examine the treatment, the mortality was considerably less.

To conclude my remarks on the comparative success of homœopathic and non-homœopathic treatment, I may briefly resume the results of the examination. The difference between those obtained in acute rheumatism by non-homœopathic treatment in five different series of cases, and by homœopathic in one series of cases, is not very great. The homœopathic mortality, compared with that of two of the non-homœopathic series, was less by 1·6 per cent. and 0·88 per cent.; and greater in other two. by ·32 per cent. and ·48 per cent. We must not, however, overlook the circumstance of the small number of cases treated homœopathically, compared with those treated non-homœopathically. Still, however, if we admit the accuracy of the homœopathic statistics, they go far to confirm the statement of Drs. Wurmb and Caspar, that in cases of acute rheumatism 'homœopathy gives exactly the same results as the expectant method.' I may remark that the non-homœopathic treatment of the cases to which I have referred was chiefly palliative; and in the most successful of Dr. Chambers' cases no medicine whatever was used.

The comparison of the results of homœopathic and non-homœopathic treatment of intermittent fever, I consider to be decidedly in favour of the latter method; and many eminent homœopathic practitioners have frankly admitted the fact. I think it is not, however, because homœopathy accomplishes less in this than it does in other diseases, but because the old school has found a drug that in the majority of cases really can cure the disease; or, in other words, that can render its duration shorter than when it is left to its own course.

If we accept without modification the results of the treatment of typhus fever in the homœopathic hospitals of Gumpendorf and the Leopoldstadt, it must be admitted that they are far more satisfactory than those of some non-homœopathic hospitals, such as those of Glasgow or London; a little more so than those of some others; and less successful than those of the hospitals of Prague and Stockholm, to say nothing of the success obtained by the hydropathic treatment of the disease.

When examining the report of Dr. Fleischmann's cases of typhus, I remarked that it lost much of whatever value it might possess, from the absence of all details or commentaries on the cases; and I showed, likewise, that there was reason to believe he had classed many cases as typhus which in other hospitals would have been classed with cases of gastro-intestinal catarrh. In discussing the comparative merit of homœopathic and non-homœopathic treatment of rheumatism and pneumonia, I pointed out circumstances in Fleischmann's own reports that should prevent us from relying on the accuracy of his diagnosis; and I have no doubt, had he given some details of his cases, as Wurmb and Caspar have done of theirs, many serious objections would have been made to his manner of classing diseases, which would have led to a considerable modification of the results of his treatment. I have already shown that the results of Wurmb and Caspar's treatment of typhus cannot be fairly compared with those of non-homœopathic practice, in consequence of their classing as typhus some cases which would be generally regarded as gastro-intestinal catarrh.

In the treatment of cholera, the homœopathic hospital practitioners seem to have had, on the whole, more success in the cases examined—from 3 per cent. at Prague to 11 per cent. in Tessier's wards at Paris—than the non-homœopathic. But the comparative results obtained in the treatment of cholera at Vienna during the epidemic of 1866, in two hospitals under the general management of the Sisters of Mercy, in which the class and condition of the patients, diet, attendance, etc., were as nearly as possible the same, completely neutralize those obtained at Paris and Prague. In the allopathic section of the Leopoldstadt Hospital, the mortality was 11·33 per cent. less than that of the homœopathic section, and 3 per cent. less than that of the Gumpendorf Hospital. I believe the treatment employed in the allopathic section was principally palliative; and the results obtained in it would seem to confirm the supposition that, by abandoning heroic plans of treatment, and heroic remedies, the non-homœopathic treatment would become much more successful than it has generally hitherto been.

I have been able to make an examination of the comparative results of the treatment of pneumonia in a more satisfactory manner than those of any other of the diseases which I have examined, owing to the number of documents relating to the subject, and to the comparative facility with which the disease can be recognised, and its progress traced.

There are some circumstances which go far, I think, to counterbalance the favourable influence that the age of most of the patients (children) of Dr. Barthez and Dr. Ziemssen must have had on the mortality. According to the observations of Gherard and Rufz, the mortality of pneumonia in children under two years of age is enormous; but it appears to me to have been exceptionally high in their cases. I believe that in children of three and even four years of age, it is considerably higher than in older ones. In a report of Dr. Caspar's[1] I find

[1] *Parallelen zwischen Allopathie und Homœopathie*, p. 34.

that the mortality of that disease during the first decennium amounts to 25 per cent., the highest in the table, with the exception of the fifth decennium, in which the mortality is 50 per cent. In the second and third decenniums there are no deaths, although they contain about five-ninths of the total number of the patients. Making every allowance, then, for the supposed advantage of the age of Drs. Barthez' and Ziemssen's patients, there can be little doubt that the mortality of their cases is far less than that of the most favourable homœopathic practice. Dr. Hughes Bennett's treatment was more successful by 2·7 per cent., and Kissel's by 1·4 per cent., than the most successful homœopathic. The mortality of Dietl's first series of cases, 7·4 per cent.; of Lebert's, 7·3 per cent.; and at the Edinburgh Infirmary, 8·3 per cent., is very similar to that of the homœopathic physicians Eidherr and Tessier, which is 7·2 per cent. and 7·3 per cent. respectively. The mortality of Dietl's second series of cases, 9·2 per cent. ; of Huss' second series of cases, 10·2 per cent.; and of those treated in the allopathic section of the Leopoldstadt Hospital during a series of six years, 12·5 per cent., approximates pretty closely to that of the homœopathic section of the same hospital in 1850, 12·55 per cent., and during a separate series of six years, 9·57 per cent. Then comes the higher mortality of Grisolle, of the Wiedner and Allgemeine Hospitals of Vienna, of Louis, etc., to which the least favourable of the homœopathic hospital treatment is far superior.

In comparing the general mortality of the homœopathic and non-homœopathic hospitals, we need not take into consideration the possibility of mistakes in diagnosis. It is only necessary that the number of cases treated and the number of deaths which occurred should be accurately reported, and that the results compared should be obtained from patients whose general condition was as similar as possible, and who were treated about the same time and in the same place. I think, therefore, the comparison of the general results of homœopathic and non-homœopathic hospital practice will be considered satisfactory, especially as it extends over a period of several years. The results of Tessier's homœopathic treatment at the Hospital of Ste. Marguerite, compared with those from patients treated allopathically in the same hospital, give 2·8 per cent. in favour of homœopathy. The injudicious activity of the allopathic treatment employed at that period probably contributed to turn the balance in favour of homœopathy.

The average mortality in the allopathic section of the Leopoldstadt Hospital during a period of six years was 4·4 per cent.; in the homœopathic section it was 3·2 per cent.; and in the hospital at Gumpendorf, during a period of seven years, 5·8 per cent. These data give 1·2 per cent. in favour of homœopathic treatment compared with allopathic in the Leopoldstadt Hospital; and 1·4 per cent. in favour of allopathy, when the results of the allopathic section are compared with those of the Gumpendorf Hospital. In the homœopathic section, the mortality was 2·6 per cent. less than in the Gumpendorf Hospital. In the one at Paris, the homœopathic treatment had about the same amount of success over the allopathic, as in Vienna the treatment of one homœopathic hospital had over the other.

In a former part of this work, I endeavoured to show that, from want of precision in its terms, the practical application of the fundamental principle of homœopathy must frequently lead to erroneous and conflicting individual opinions. I cited numerous homœopathic authors, who had frankly admitted the great difficulties which they had encountered in selecting the proper remedies for disease; and while examining the results of homœopathic practice, I several times had occasion to point out incidentally facts which confirmed the truth of my statement. Thus Drs. Wurmb and Caspar, for example, have stated that, in treating intermittent fever, they made as many wrong guesses as right ones (77) in selecting the proper remedies for the different cases. The same authors, after a rather elaborate examination of the action of two remedies, arsenic and phosphorus (which were regarded by Fleischmann and others as a sort of universal specifics, the former in typhus, the latter in pneumonia), have affirmed that they cannot be considered as homœopathic remedies in these diseases. The opposite views of these authorities in homœopathy respecting the action of the two drugs may perhaps be satisfactorily explained by a statement to which Dr. Rapou, with the greatest good faith, called the attention of his brethren, and which I noticed in a former part of this work, 'that a drug may act homœopathically to a disease in one part of a country and not in another,' or, in other words, that it may cure a given disease in one part of a country and not be able to do so in another. But what is more to the point, he said 'that a remedy homœopathic to a disease in a given place may cease to be so in the same locality a year afterwards.'

I likewise had occasion to show that one of the secondary principles of homœopathy (the necessity for individualizing disease) of which the importance is so strongly insisted on even at the present day, had been practically set aside by Fleischmann and others in the treatment of typhus and pneumonia. Their homœopathic brethren have remonstrated with them; but the results of their treatment show that the principle can be set aside with impunity.

Before finishing my summary, an important question naturally presents itself: Have I been able to arrive at a decisive conclusion regarding the comparative value of homœopathic and non-homœopathic treatment? Certainly not. The small number of suitable homœopathic hospital reports hitherto published has enabled me to make only a limited comparison between the results of the two methods.

The reports of Tessier I consider as trustworthy as those of any allopathic practitioner. Those of Drs. Wurmb and Caspar are trustworthy; but in consequence of some peculiarities in their manner of classing cases of typhus, cholera, and probably also of pneumonia, we are sometimes left in doubt as to the precise nature of some of the diseases to which they refer. Fleischmann's reports, for reasons already assigned when examining the treatment of pneumonia and typhus fever, I consider much less trustworthy.

Still I think one may safely draw two important conclusions from these limited and imperfect data: 1*st*, that in the diseases examined, with the exception of intermittent fever, the results of homœopathic treatment in hospitals have been about equal to the most satisfactory non-homœopathic: 2*d*, that the results of homœopathic and non-homœopathic treatment, in which little or no medicine was employed, have been nearly the same; or, in other words, that drugs in the doses usually administered by homœopathic practitioners have not appeared to exercise any decided influence on the progress of disease.

Many of the opponents of homœopathy have ascribed the success of its treatment to the influence of the imagination; others to the dietetic and hygienic means employed by homœopathic physicians, and some with more reason to the influence of the *vis medicatrix naturæ.* To the first two objections homœopathists have satisfactorily replied by saying, 'If you really believe that diet or the imagination can accomplish so much, you act very inconsistently by not availing yourself in your own practice of such powerful means.'

To the third objection it is impossible for them to give a complete and satisfactory answer in the present state of pathological knowledge; nor will it be possible to do so until we have become more familiar with the natural progress of disease, and have thus acquired a fixed standard of comparison that will enable us to say whether or not, in any given case, the phenomena observed are produced by the organism itself, or by some drug or other artificial cause. Until this standard be acquired, however, homœopathists cannot affirm that in any given case of disease recovery was caused by the action of their drugs; to do so would be to assume the very point that requires to be proved. They must prove, at least, that the results furnished by the expectant method are fallacious, and that the infinitesimal doses of drugs possess curative power.

One of the most scientific of the homœopathic physicians, the late Dr. Wurmb, saw clearly that this was one of the weak points of homœopathy. He says: 'There is a far more important and far more difficult question to answer—How stands it with homœopathy in relation to the expectant method? or what does nature, and what can art do? This is a question of life or death for homœopathy.' 'Wie verhält sich die Homœopathie zur expectativen Heilmethode, oder was thut die Natur und was vermag die Kunst? Sie ist eigentlich die lebensfrage der Homœopathie.'

But although our knowledge of the natural course of disease is still very limited, yet, as I think the comparative results of homœopathic and expectant treatment are so similar, we may justly regard them as produced by the same agencies—the restorative resources of the organism, aided by hygienic and dietetic means. In a former part of this work I have shown that Drs. Wurmb and Caspar found them to be so similar in acute articular rheumatism, that they considered the recoveries from that disease, under homœopathic treatment, to be owing to the curative resources of the organism itself. And if the results of the expectant and homœopathic treatment are so nearly the same in the important diseases in which I have been able to compare them, I think I may safely venture to assume that they will likewise be found similar in other diseased states. I hope increased knowledge of the natural course of disease will soon enable medical men to give a more decided opinion on this point.

When examining the subject of the medicinal action of infinitesimal doses, I found reason to conclude that—with the exception, perhaps, of the first centesimal, and the first, second, or third decimal dilutions of active medicines—they could not have any curative action; and that conclusion, arrived at by *à priori* reasoning, has thus been so far confirmed by the results of their use in disease. We may therefore regard almost every case treated homœopathically as an illustration of the natural course of disease, and we may consequently turn it to account in examining that subject. We shall thus find an immense collection already made of cases illustrative of the natural course of every variety of malady, although unfortunately in many of them the requisite details have not been given.

Before concluding this part of my work, I would offer a few remarks to those medical men who still have great faith in the therapeutics of the old school, and who still talk of treating diseases according to what they call the principles of scientific medicine. I will assume only—what will not probably be denied—that no reasonable objections can be made to the reports of Tessier, to those which I have given of the general results of homœopathic hospital practice, or to those of Eidherr. In the recoveries that took place under homœopathic treatment, according to these reports, the drugs employed either cured the diseases, or they did not. If they cured them, then the Materia Medica of the old school must be put aside at once; if they did not, it must still be put aside, since the most successful results, with few exceptions, were obtained without the aid of drugs. In short, the sad conclusion is inevitably forced upon us, that the Materia Medica of the old school, the result of the accumulated experience of ages, is a worthless, nay more, as it has been hitherto frequently employed, a noxious mass of what was once regarded as health-restoring drugs. The truth of this conclusion cannot be gainsayed; and no conscientious and intelligent medical man can ponder over it, without resolving to abandon the chaotic polypharmacy of the old school, and trying to ascertain by proper investigations what drugs really do accomplish in the cure of disease.

PART III.

WHEN I consider how many talented men have vainly attempted to give therapeutics a more scientific form, I naturally approach this part of my work with great hesitation. But as it appears to me that, from comparatively recent pathological and therapeutical investigations, we are now better prepared than we were thirty years ago to point out the chief causes of the unsatisfactory state of therapeutics, as well as by what means their removal can be best accomplished, I shall offer some remarks,—

1*st,* On the aid that physiology, pathology, and chemistry can give to therapeutics.

2*d,* On the necessity for acquiring accurate knowledge of the natural course of diseases.

3*d,* On the best means for ascertaining more correctly the curative properties of drugs.

4*th,* On the importance of applying our knowledge of the influence of mind on the body, in a more systematic manner than has hitherto been generally done in therapeutics.

CHAPTER I.

ON THE AID THAT PHYSIOLOGY, PATHOLOGY, AND CHEMISTRY CAN GIVE TO THERAPEUTICS.

SOME of the most eminent medical men of the present day, particularly of those who belong to the physiological school, regard physiology and pathology as the only proper foundation of rational therapeutics. I think this opinion requires to be modified. No doubt it is only by means of physiology and pathology, aided by chemistry, that we can attempt to explain the action of medicines; but even at the present day our knowledge of these subjects is far too limited and imperfect to allow us to give more than a very general and imperfect explanation of the action of drugs: in fact, there is no medicine of whose action a complete and satisfactory account can be given. Besides, the most complete knowledge of physiology, pathology, and chemistry would not enable a person ignorant of the properties of drugs to form the slightest idea of the effects that any of them would produce when administered to a sick person. That information must be obtained by special researches.

The opinions of medical men on therapeutics have always been strongly influenced by the physiological and pathological notions of the day, and, like them, they have been constantly changing. During the present century, the application of the microscope and other improved means of research to physiological and pathological investigations, has modified so completely our previous knowledge of these subjects, and has added so largely to it, that at present it is far more extensive, and probably also more accurate, than it formerly was. But in this apparent increase of our knowledge, we must not overlook the fact that we have acquired but comparatively few fixed points, at least as far as regards the details of the different functions and processes of the body. The discovery of to-day but too frequently involves the necessity of abandoning our previous notions on the subject to which it relates; and at present there are but few points of detail on which the opinions of investigators completely harmonize. What a large portion of every work on physiology and pathology consists merely of summaries of individual opinions; and how small would be one that would give us only the well-established facts of these two branches of medical science!

The difficulties inherent in the study of the phenomena of living bodies are far greater and more numerous than those which are met with in the investigation of any other branch of human knowledge.

Organic chemistry has already rendered great services to physiology and pathology, and it will undoubtedly render them still greater. In fact, it is indispensable to their progress; but until it assumes another form than it has at present, we cannot rely much on its results. It is to organic chemistry, however, that physiology and pathology look for the solution of many of their most important problems.

We cannot overlook the important aid that chemistry has given to therapeutics during the last fifty years, by analyzing drugs and isolating their active constituents, and thus enabling us better to study their action. We must not forget, however, that a knowledge of the medicinal properties of the different constituents of drugs has not been furnished, properly speaking, by chemistry. It has been acquired sometimes accidentally, but generally by trials on animals in the first place, and afterwards, on the human subject. In all carefully made trials with medicine, whether on the healthy or on the sick, the aid of chemistry is more or less required. In many cases it is only by means of its re-agents that we can form some idea how a medicine acts, or of the sphere of its action. It is to be hoped that, with the disappearance of polypharmacy, the assistance of chemistry in pointing out incompatible medicines will cease to be required.

From the conflicting views which have been held respecting the phenomena of living bodies, it is not surprising that we have hitherto made but little progress in tracing their relations to one another, or, in other words, in discovering the laws by which they are governed. But however imperfect our knowledge of the healthy and diseased states of the organism may be, there can be no doubt that we are now much more familiar with the phenomena of disease than we were at the beginning of the century. Our diagnosis has become far more precise; and knowing better in what diseased states consist, we understand more clearly what we require to do with the agents which the Materia Medica has placed at our disposal.

An improved physiology and pathology, then, may yet render great services to therapeutics, by enabling us to take clearer and more accurate views of the manner in which drugs produce their various effects on the system; and by explaining the order and connection of the phenomena of disease, render it easier for us to determine in any given case whether the phenomena observed be part of its natural course, or if they be owing to the action of extraneous causes. But the natural course of disease is a part of pathology so important in its relation to therapeutics, that I shall speak of it in a separate chapter.

CHAPTER II.

ON THE NATURAL COURSE OF DISEASE.

ALTHOUGH no one can doubt that the phenomena of disease, like those of health, conform to certain laws, the insurmountable difficulties occasioned by their obscurity and complexity have hitherto rendered the attempts of pathologists to trace the connection of cause and effect amongst them very fruitless and unsatisfactory, except in some cases in which they present themselves more or less directly to the cognizance of our senses. In every case of recovery in which medicine has been used, the cure must have been accomplished in one of two ways, either by the curative power of the organism itself, or by the same power combined with the action of drugs. Unless we know accurately the phenomena which either of these agents may cause, we cannot pretend to assign its share to it in the production of the results observed in any case that we treat. If we knew well, however, the action of one of them, then by exclusion we could attribute the production of the effects not caused by it to the influence of the other. But as we have only a very vague and limited knowledge of what the resources of the organism can accomplish in disease—or, in other words, of its natural course—and as we know but little of the positive action of drugs, it is impossible to speak very decidedly of the effects of our treatment in most cases: our opinions on that point can only pretend to more or less probability. At present I shall confine my remarks to the former of these topics; I shall treat of the other in the next chapter.

I have already had occasion to advert to the important pathological fact,—the natural tendency of the diseased organism to return, under favourable circumstances, to a healthy state. The slighter the deviation from it, the more strongly does that tendency manifest itself. In proportion as the diseased state increases, the disposition to return to health becomes less, until, after having reached a certain degree of severity, its natural course is no longer to diminish, but to increase, until death ensues. In the present state of our pathological knowledge, we cannot say with great certainty whether the natural course of many forms of disease be to return to a healthy state, or to continue to increase till death occurs. We frequently enough see mild cases become more and more severe and complicated till life be destroyed; and again, on the contrary, cases of hopeless severity gradually restored to health. As we become more familiar with the natural course of disease, we shall probably be able to account for these exceptional cases. In the meantime, we must consider this tendency of the diseased organism, whether restorative or destructive, as an ultimate fact, of which consequently no explanation can be given; and when we make use in this work of such expressions as, 'Nature cures the disease,' it is simply to express that ultimate fact, without superadding to it the notion of some mysterious influence or entity called vital power, vital force, etc. In the first chapter of this work I have attempted to show that such terms, at least when employed in their usual sense, besides implying mistakes in philosophy, add to the obscurity which they are intended to remove.

Unless we have such a clear and correct knowledge of the natural course of disease as to be able to form a standard with which we can compare the phenomena observed during the treatment of any case, it will evidently be impossible to distinguish accurately what are produced by the organism itself from those caused by the action of drugs, especially if our knowledge of the latter be very imperfect; and to attempt without such a standard to determine what effects have been caused by drugs, would be to expose ourselves to unavoidable errors, as experience daily shows us. But it may be said it is impossible to form such a one, on account of the variety of the symptoms, and of the order of their sequence in different cases even of the same malady. A similar objection might be made to the general descriptions of disease which are given in works on practical medicine; for, however uniform in its character disease may be, the general description of it will not correspond closely with the symptoms that manifest themselves in individual cases. It is a disadvantage that cannot be avoided, but it may be lessened by giving numerous illustrations of individual cases.

For our special purpose, it does not appear to me to be so necessary to have a general description of disease that will represent accurately the symptoms as they present themselves in particular cases. It will be sufficient, I think, to have all those that may present themselves in the course of any diseased state arranged as nearly as possible in the order of their development, and in different groups, according to the greater or less frequency of their occurrence. Those which are always or almost always present should be placed in the first group, and the others in one or more groups in proportion to the frequency of their occurrence. In this way it will be easy to have a standard or general description of the natural course of disease, which will enable us to say, in any given case of treatment (especially if, in addition to that, we possess a good knowledge of the properties of the drugs employed), what symptoms should be referred to causes inherent in the organism, and what to the action of drugs.

At every period of medicine, medical men have admitted the existence of the curative resources of nature, and many have recognised them as the basis of their treatment. It is only during the last twenty-five years, however, that the natural course of disease has become the subject of methodical inquiry. The Austrian school of medicine has the great merit of having taken the initiative in this most important branch of medical investigation, and of having given us the results of its observations made on a gigantic scale on formidable diseases, as pneumonia, typhus fever, etc. Physicians in other parts of Europe have since undertaken similar researches; but in our own country, it is only during the last few years that some medical men have turned their attention to this subject. It cannot yet be said, however, that we possess a complete and satisfactory account of the natural course of any diseased state, although we have now become much more familiar with the leading phenomena of some of them, and have learnt quite enough to be fully convinced that the Materia Medica and therapeutics of the old school must be entirely reconstructed.

More than thirty years ago, I not unfrequently had occasion, in the interior of Russia, to see severe cases of such diseases as fever, dysentery, pneumonia, rheumatic fever, etc., which, from the great difficulty of procuring medical aid, were allowed to run their course without the use of drugs. I was much struck by the recoveries that took place in many of them. They showed me that nature could cure diseases which I had hitherto supposed to be almost inevitably fatal without medical aid; and they appeared to go far in explaining the success of homœopathy, which for several years had been attracting some notice in that country. As I supposed the infinitesimal doses of its drugs were inert, I concluded its apparent success could be ascribed only to the *vis medicatrix naturæ;* and if my views were correct, that carefully detailed cases of homœopathic treatment would be invaluable sources of pathological knowledge, by showing what nature could accomplish without the use of drugs.

Of late years, since my attention has been more closely turned to the subject of this work, I have become more strongly than ever convinced that our ignorance of the natural course of disease, and of the action of drugs, has been the most fertile cause of erroneous opinions in therapeutics, not only in ancient, but likewise in recent times. Numberless cures of every variety of disease, ascribed to the action of remedies which at present are regarded as inert, or at least incapable of producing the effects ascribed to them, have been recorded in the annals of medicine. As these cures cannot be ascribed to the action of the medicines employed, they must have been brought about by the natural operations of the organism. The truth of this opinion is amply confirmed by what we already know of the natural course of some of them.

Since a large proportion of diseases, then, may get well without the use of drugs, it is evident that a corresponding number of erroneous opinions respecting their properties must be formed by those who regard all recoveries as the necessary results of their action. It is quite natural that physicians unacquainted with the curative resources of the organism should ascribe to the action of the medicines employed any favourable results that might occur after they had been administered. *Post hoc,* *propter hoc*, is the all-pervading fallacy of therapeutical investigation: in fact, it could scarcely be otherwise.

As a practical illustration of my statement, that ignorance of the natural progress of disease is one of the chief sources of therapeutic error even at the present day, I may refer to the discussions which occurred at the Academy of Medicine on the treatment of acute rheumatism, during which the power of curing most successfully that disease was alternately accorded and denied to a great many remedies. Had the medical men who took a part in them been acquainted with the natural course of the disease, such discussions could not have arisen; for no one would have thought of ascribing to the action of any particular drug what would have taken place, at least as satisfactorily, without using it. With a knowledge of what nature could accomplish in the way of a cure, there could not have been much discussion about the comparative merit of remedies.

The only objection that I have heard urged against this part of the plan which I recommend for giving a solid basis to therapeutics, is one which at first sight appears formidable enough: 'That no conscientious medical man can witness the progress of a serious disease without using the means placed at his disposal to remove it, or at least to alleviate some of its symptoms.' If by means we are to understand drugs, the objection assumes what cannot be admitted, except in a small number of maladies,—that we possess drugs capable of removing them. I think I have sufficiently shown in the second part of this work, that *the most satisfactory results have been obtained by the expectant method, or by simply palliative remedies, in some formidable diseases, such as pneumonia, acute rheumatism, typhus*, etc. The use of mild palliative medicines to relieve certain symptoms of a disease, would not, probably, modify much its natural course. Besides, those who make such an objection would certainly refuse to accord any curative action to the doses of medicine usually employed by homœopathic practitioners. In homœopathic practice, then, we have innumerable examples of every possible variety of malady left to its natural course; and the results,[1] as I have shown, have

[1] When speaking on this subject with some eminent physicians of the old school, I have been surprised at their great incredulity with regard to

been such that the most scrupulous physician need not hesitate to do what homœopathic practitioners have virtually done —place the patients in the most favourable circumstances possible, as far as regards diet and hygiene : the rest will be done by the organism itself. Homœopathists may object to this remark; but until they can show, as I have said in another part of my work,[1] that under these circumstances the course of disease would be less favourable than under homœopathic treatment, it cannot be affirmed that their treatment cures the disease.

the reported results of homœopathic practice. It is true that many untrustworthy reports have been published by homœopathists as well as by other medical men ; but it was remarkable enough, that not one of the gentlemen to whom I refer had ever seen a case treated homœopathically. Were they to observe homœopathic practice for some time, as I myself have done, their scepticism would probably be modified, and they would find that a large number of important diseases may get on as satisfactorily without as with the use of drugs.

[1] See page 202.

CHAPTER III.

ON THE NECESSITY FOR PROVING DRUGS.

DURING the last century, but especially since the beginning of the present, many of the most distinguished and experienced physicians of different countries have denounced the numberless errors and radical defects of the *Materia Medica.* At the present day its unsatisfactory state is so generally admitted, that my statement requires no confirmation. Several authors have suggested that it should be entirely reconstructed: but how can that be accomplished?

With two or three exceptions, which I shall immediately notice, no physician of the old school who has turned his attention to this subject has been able to suggest any satisfactory practical means for removing this deplorable state of things. The summary of the suggestions which have been made is: 'Carefully conducted trials with medicine in well-defined cases of disease.' This plan has been repeatedly carried out with the greatest care, and with all the improved resources of modern medicine; but on referring to works of the day on therapeutics, or the *Materia Medica,* we find that little progress has been made in ascertaining the real curative properties of drugs.

During the latter half of the last century, several medical men proposed, as the best means for ascertaining the properties of drugs, that they should be first tested on healthy individuals; and afterwards, when a knowledge of their properties had been thus acquired, that they should be employed in the cure of disease. Störck, of Vienna, made a number of trials on himself with colchicum and other poisonous plants, of which he has given an account in two small works.[1] About the same period,

[1] *Libellus de colchici autum. radice,* Vienna 1763; and *De Flammula Jovis,* Vienna 1769.

Mr. Alexander of Edinburgh published an account of some trials which he had made on himself with different drugs;[1] but although his small work is well worthy of perusal, I believe few medical men of the present day are acquainted with it. The celebrated Haller, in his preface to the Swiss *Pharmacopœia*,[2] recommended the proving of medicines on the healthy, and, guided afterwards by the effects observed, the making trial of them in disease:—'Nempe primum in corpore sano medela tentanda est, sine perigrina ulla miscela.' 'Inde ad ductum phænomenorum in sano obviorum, transeas ad experimenta in corpore ægroto.' But the medical man who recommended most energetically the proving of drugs on healthy individuals was Hahnemann.

In an essay written with great ability, and published about the beginning of the present century, he made a vigorous and damaging onslaught on the *Materia Medica* of the day. Although his views on the subject cannot be considered original, and although a spirit of exaggeration (Hahnemann's besetting sin) pervades the essay, every candid medical man will admit that the opinions which he has expressed in it are essentially true. He proposed to remedy the defects he so fearlessly exposed, and to place therapeutics for the future on a more secure basis, by energetically carrying out the plan he had formerly proposed, of testing the properties of drugs on healthy individuals before attempting to employ them in the cure of disease.

I consider that plan to be one of the most valuable innovations that has ever been made in practical medicine. As I have said on a former occasion, the *Materia Medica* owes much to Hahnemann for the untiring energy and for the judgment which he displayed in carrying out his earlier provings. Unfortunately, influenced by theoretical notions, he afterwards adopted the method of making his trials with infinitesimal doses of drugs; and from that period, his provings became a still more worthless farrago of opinions than the *Materia Medica* which he had so violently condemned.

Most of the associations subsequently formed by medical men for proving medicines adopted a more rational plan than

[1] *Experimental Essays*, by W. Alexander, Edinburgh 1768.

[2] *Pharmacop. Helvetica*, Basel 1771, p. 12.

that followed by Hahnemann. The Allopathic and Homœopathic Proving Associations of Vienna, especially the latter, have made some elaborate provings. I think the former, whose labours were continued for a short period only, committed a great mistake in excluding from their list of effects produced by drugs all those symptoms which were not remarked by the generality of the experimenters.[1] The latter, after carrying on its provings for a number of years, resolved at one of its meetings in the autumn of 1864 to discontinue them. It appears to me that some of its members, in making their provings, have not unfrequently committed the serious mistake of ascribing casual phenomena to the action of the drug that was being proved at the period of their occurrence.

It is remarkable enough, that although several eminent physicians of the old school have strongly approved of the plan of proving medicines on healthy individuals, only three associations formed by non-homœopathic physicians have made some provings worthy of notice—Professor Jörg's, the Allopathic Proving Society of Vienna, and that formed by some of Rademacher's followers.[2] To homœopathic practitioners the proving of drugs is indispensable, since their practice is based on it; yet even amongst them many fruitless attempts have been made to organize proving associations.

The only plan which has hitherto been almost exclusively employed, in order to acquire a knowledge of the medicinal properties of drugs, has been by making trials with them in disease. At first sight, it appears to be the only rational method which can be employed for that purpose; but unfortunately the results obtained in that way have been very unsatisfactory. I have already shown that, if we possessed a complete knowledge of the action of one of the two agents engaged in

[1] We must accept as well-established facts, 1*st*, That a given dose of a drug often produces very different effects on different individuals, and even on the same individuals at different times; 2*d*, That considerable differences are often remarked in the action of different doses of the same drug.

[2] There is certainly something repulsive in proving drugs, especially to practitioners of the old school, who are not strongly impressed with the importance of the practice; and I know by experience it is no easy task to persuade them to engage in it.

the cure of every case of disease in which medicine has been employed, it would not be very difficult to assign to the other its share in the production of it. But with the limited knowledge which we possess of the action of both agents, the question of the curative operation of any drug in most diseases is at best, even at the present day, but a calculation of probabilities. The accumulated experience of ages has not succeeded in giving us much definite and well-grounded knowledge respecting the curative properties of drugs. Must we still continue, then, to follow the plan which has proved to be so fruitless? Is there no other means besides the study of the natural course of disease and trials with medicines on the sick, which will enable us to gain a greater amount of accurate information respecting the properties of drugs?

It appears to me that we can simplify much the study of the action of drugs, by testing them in cases from which the perplexing and embarrassing phenomena of disease can be completely excluded, that is, by trials with them on healthy individuals. Hahnemann said justly, that drugs must cure disease by virtue of the same properties which enable them to produce it in the healthy; and I shall soon have occasion to show that, in general, their action is similar both in health and in disease. Even if their physiological action were more or less modified in certain diseased states, it seems very probable that the important point of the sphere of their action, which can be best ascertained by trials on the healthy, will not be materially changed in disease. In short, the utility of trials with drugs on the healthy is so manifest that no one can doubt it.

I shall here offer a few remarks on the manner in which drugs should be proved. There are two circumstances which must never be overlooked by those engaged in testing their action: 1*st*, That one drug only should be proved at a time. If two drugs were administered at the same time, it would be impossible to say in many cases to which of the two any effects observed should be attributed. The more complex the medicine used, the greater would be the difficulty of referring any effects observed to their proper causes. 2*d*, Whilst testing the action of drugs, the prover should carefully avoid whatever might exercise a disturbing influence on the various functions of the body, such as great mental excitement or bodily fatigue, the use of articles of food or drink known to disagree with him. Wine, tea, or coffee may be moderately used, if the prover has been accustomed to their use. During the trial, he should avoid making any great change in his diet or habits.

Provings are generally carried on more satisfactorily when several individuals (say from six to ten) are associated for that purpose. Every person should be more or less acquainted with the properties of drugs, and it would be desirable that he should observe the state of his health for a few days before commencing the use of the drug. If not satisfactory, the proving should not be commenced until he feels quite well again. A short description of the physiological condition of the prover should be made. If he has any idiosyncrasy, it should be noted. It should likewise be stated whether or not he is subject to attacks of any particular disease.

The prover should begin his trial of the drug by taking, in the morning, or before going to bed, one-fourth or one-sixth of the smallest dose indicated in the *Pharmacopœia.* The dose should be daily and gradually increased, until the largest considered advisable be taken. If any marked effects be produced at any period of the trial, the dose should not be increased for two or more days; or the use of the drug might be given up for a similar period, to be resumed in the same doses at which it was left off.

The drug selected for trial should be proved in different forms. The best for testing the properties of vegetable medicines is that of freshly expressed juice; but it cannot in general be easily procured. The next best form is that of a fine powder or of a tincture. When the requisite dose of a tincture is considerable, its employment should be avoided, as the quantity of alcohol taken might produce effects likely enough to lead to erroneous conclusions. In some cases, an infusion will be a better form than that of tincture. In the form of extract, even when carefully prepared, the constituent elements of the drug are so much changed in many cases, that its action can scarcely be expected to be exactly the same as that of the original medicine.

It has been recommended that the prover, while testing a drug, should carefully watch for any effeet it may produce. I think it would be better, however, that his attention should not be too closely directed to his sensations, as we know how readily his imagination may mislead him under these circumstanees. It will be sufficient, I think, for all practieal purposes, that the experimenter pass rapidly in review, several times during the day, the state of his different functions. Every morning and evening, any ehanges observed in the funetions of the different organs, as well as any symptoms that may have been remarked since last report, should be written down. The quantity and quality of the urine, as well as the state of the pulse, bowels, etc., should be earefully noted. In testing the properties of a drug, trials should be made on individuals of both sexes and of different ages. Every intelligent prover will modify the plan for proving drugs, of which I have given a brief sketch, aeeording to the partieular objeet he may have in view.

Many attempts have been made to acquire a knowledge of the properties of drugs, by making trials with them on some of the inferior animals. I believe an association of medical men was formed a year or two ago at Paris for that purpose. In general, however, I think no great advantage ean be derived from such experiments, except when we wish to form some general idea of the aetion of any substance whieh we supposed to be possessed of medieinal properties, or when we require to ascertain in what doses it may be safely given to human beings. It is well known that some medieines act very differently on some of the inferior animals from what they do on man. Some animals use with impunity substances that are poisonous to man; and others, again, innoeuous to man, are injurious to some of the inferior animals.

Besides, from experiments on the inferior animals we can never aequire a knowledge of subjeetive symptoms, which are often of great use in enabling us to form eorrect notions of the aetion of drugs. In some eases, it is true, we may guess with more or less eertainty the nature of the sensations of the animal; but the most sueeessful guess can never possess the precision and certainty of verbal deseription. In short, we ean never be certain that a drug will aet on the human subjeet as it has acted on some inferior animal, until trials have been made with it on the former.

As the chief object that we have in view in making them, is to ascertain the action of drugs on the human subject, it would evidently be better to make them directly on man, without losing time in making incomplete, and possibly misleading, trials on inferior animals. Having once acquired a knowledge of the action of a drug on the human subject, we might be enabled to form clearer and more correct notions respecting it by comparing it with the results obtained with the same drug on some of the inferior animals. And in those cases in which the effects of a drug on some animal are very similar to those which it produces on man, we might gain some important information by giving comparatively larger doses of it to the former than we could venture to give to the latter.

It has been objected to the plan of proving drugs on healthy individuals, that as they must act differently in a diseased from what they do in a healthy state of the body, we cannot infer from their action in the former case what effects they will produce in the latter. This objection appears at first sight to be well founded. Drugs produce their effects on the healthy, under conditions so different from what they must often do on the sick, that we may expect to find their physiological action more or less modified on the latter; that some of the effects caused by them in health will cease to manifest themselves in disease, or that some entirely new ones will be produced. Experience alone can determine that point. As far as our present knowledge of the subject goes, we find that modifications of their action on disease occur less frequently than might have been anticipated; and that the differences between the more marked effects produced by drugs on the healthy and on the diseased organism, are not greater than between those produced by them on different healthy individuals. Opium, for example, acts on the nervous system, on the intestinal canal, and on various secretions, in the diseased as well as in the healthy states of the system. Tartar emetic and ipecacuanha produce sickness and vomiting in disease as well as in health. Castor oil and other purgatives act similarly in both states. In disease, digitalis affects the heart's action, the brain, and the kidneys, as well as in health. The action of arsenic is as marked in the diseased state as in health; and the same can be said of mercury. As I stated in the first part of this work, M. Briquet, who is well acquainted with the action of quinine, affirmed that its physiological and pathological effects were quite the same; and his opinion has been confirmed by the experience of other physicians. Belladonna presents its characteristic effects in disease as well as in health; and the same may be said of chloroform, one of the most valuable agents ever added to the Materia Medica.

One of the most frequently remarked modifications of the action of drugs in disease, is a difference in the amount of the dose required to produce a certain effect. For example, much larger doses of opium are requisite to produce narcotic effects in cases of delirium tremens, or in tetanus, than in a state of health; and, on the other hand, doses of cantharides, which could not produce any notable effect on the urinary organs in health, may affect them very sensibly when they are irritated or inflamed.

It has been said by homœopathists, that the proving of medicines cannot be of any use to medical men of the old school; 'for how can we imagine, say they, the *contraria* of such diseases as gout, ague, epilepsy, small-pox, cholera, etc.?' It is quite certain that medical men of the old school have no guiding principle such as *similia similibus* is supposed to be by homœopathists, which enables them to know, from the results obtained by proving a drug, in what diseased states it may be usefully employed. At the present day, *contraria contrariis* is seldom, if ever, referred to as a principle of treatment, although it forms the basis of that of a considerable number of diseased states. In these cases, however, the treatment is directed rather against one or more symptoms of the disease, than against the disease itself.

A priori, there is nothing unreasonable in the principle; but we know by experience, that although in some cases it can be satisfactorily applied, it does not admit of very general application; nor can it be relied on even in those cases in which drugs can produce a *contrarium* to the diseased state. A solution of the sulphate of atropine applied to the conjunctiva causes dilatation of the pupil; and its application is of great utility in iritis, by preventing the contraction of the iris, and the consequent possibility of its forming adhesions with the surrounding parts. In many cases of anæmia, preparations of iron have a decidedly beneficial effect. When there is great prostration of strength, stimulants in some form or another are habitually employed. When congestion of blood to a part occurs, cold applications are frequently useful, as in congestion of blood to the head in fever; but in cases of frost-bite, warm applications would cause the greatest harm. In paralysis, the results of the use of nux vomica or strychnine, which produces a state of the nervous system quite contrary to that of the disease, have been unsatisfactory, although practitioners have been repeatedly induced to try it in such cases. Besides, on the principle of *contraria contrariis,* it is impossible to account for the action of such drugs as quinine, iodide of potassium, colchicum, etc., the beneficial influence of which in certain diseases is most marked. In short, even at the present day, the methods of treating almost all important diseases are so various, and often so opposed to one another, that they are evidently not based on any fixed principle.

Many physicians appear to hold the erroneous opinion, that a drug acts in the same manner on all healthy individuals. Every one who has had occasion to make trials with them must have remarked differences in the effects produced, even with the same doses of a drug on different individuals, and, though more rarely, on the same individual at different times. These differences are generally more marked in the less constant effects of the drug; but I have likewise observed them in their more marked and characteristic ones. Thus, I have seen large doses of ipecacuanha administered to individuals without producing even sickness; and it not unfrequently happens that narcotic doses of opium produce a state of feverish excitement, with short intervals of drowsiness instead of regular sleep. Even in the few trials with sulphur and quinine, which I have related in a former part of the work,[1] considerable differences were remarked in their action on the different provers. It is not improbable, therefore, that future observa-

[1] See pp. 17 and 38.

tion may yet show that the pathological, like the physiological action of drugs may vary more or less on different individuals.

Practitioners of the old school have hitherto paid so little attention to the proving of medicines, that we need not be surprised if, as yet, therapeutics has derived but little benefit from what they have done in this branch of medical investigation. During the present century, however, some medical men have succeeded in introducing several valuable remedies into practice, such as iodine, iodide of potassium, hydrocyanic acid, strychnine, chlorate of potass, chloroform, etc., although only a very limited knowledge of their physiological action had been obtained by means of some incomplete experiments made principally on animals.

It cannot be doubted, then, especially as the general action of drugs appears to be more or less similar in disease to what it is in health, that, with a knowledge of their physiological action, we should be much better prepared to undertake the investigation of their influence on disease than if we were entirely ignorant of their properties. I would here call the attention of my readers to a work of great merit on the action of some medicines,[1] in which the plan of proving them has been largely carried out both on man and on animals. I think no one can peruse it, without being convinced that the proving of drugs must be of great utility to therapeutics, both by giving us clearer and more definite ideas respecting their action, and likewise by suggesting in what diseases trials should be made with them. It is to be hoped that Dr. Harley's example will induce many medical men to undertake similar researches.

Since medicines have been more frequently and carefully tested on healthy individuals, it seems to be established that every drug has a certain sphere of action; and that although several of them may act on the same organs or tissues, considerable variety is observed in their effects. Belladonna acts on the brain and its nerves, on the heart, on the kidneys, on the mucous membrane of the throat, and not unfrequently on the skin. The action of digitalis is most marked on the brain, heart, and kidneys; but although there is considerable simi-

[1] *The Old Vegetable Neurotics*, by John Harley, M.D., 1869.

larity in the sphere of action of these two remedies, their general effects are different. Mercury acts in a marked manner on the mucous membrane of the mouth and salivary glands, on the mucous membrane of the intestinal canal, and, when its action has been long continued, on the brain and nervous system. On the last-mentioned parts the action of lead is similar to that of mercury, but the characteristic muscular tremor produced by the former is not remarked among the effects of the latter drug; and the paralytic state of the muscles, which may be produced by both, affects different classes of muscles, according as the one or the other of the two drugs is employed. It is to be hoped that a more energetic and extensive investigation of this important subject will yet lead us to some general conclusions, which will give more certainty to the results of our treatment of disease, and consequently a more scientific form to therapeutics in general.[1]

Another result of the general proving of drugs will be the removal from the *Materia Medica* of a number of articles whose presence only serves to obstruct our advance towards a more rational system of therapeutics. I formerly stated that Mr. Alexander tested four different drugs on himself. He found two of them, castoreum and crocus sativus, to be altogether inert, or nearly so. He says: 'And were the whole articles contained in the *Materia Medica* to undergo the same scrutiny, I am very much afraid that more than a proportionate number of them would be found equally insignificant.' Subsequently Dr. Jörg, with the aid of several members of the proving association, tested the former drug in a more complete manner than Mr. Alexander had done. He arrived at the same conclusion as the Edinburgh practitioner, and suggested that castoreum should be removed from the list of drugs in the *Materia Medica*. It is surprising that, notwithstanding these

[1] I believe the important fact that drugs possess elective affinities for certain organs or tissues will form the basis of our future therapeutics. There can be no doubt whatever that properly conducted and sufficiently numerous trials with them on healthy individuals will give us as clear and definite ideas as can be obtained respecting the sphere of their action; and although in many cases the nature of it may be considerably modified by disease (experience alone can give us that information), it is not probable that the sphere of it will be much changed.

experiments, the editors of the *British Pharmacopœia* should have retained castoreum in the list of drugs. I think it may be safely assumed, if a drug produces no perceptible effects when properly tested on healthy individuals, that it will prove equally inert in disease. MM. Trousseau and Pidoux have expressed a contrary opinion in their work on Therapeutics; but I doubt if they could point out any drug whose action can be shown to support their view.

CHAPTER IV.

ON THE IMPORTANCE OF ATTENDING SYSTEMATICALLY TO THE STATE OF THE PATIENT'S MIND IN THE TREATMENT OF DISEASE.

THE last subject on which I shall offer a very few remarks is the influence which the mind exercises on the functions of the body, both in health and disease. My object is not so much to call attention to what every medical man knows, as to point out the importance of turning that knowledge to practical account in the treatment of disease.

The annals of medicine contain innumerable cases of disease in which, after the administration of totally inert drugs, recoveries took place which were naturally ascribed to their supposed action. In former times, too, when faith in charms, amulets, and other superstitious customs prevailed, the number of cures supposed to have been produced by these means was very great. All the cases to which I have referred were undoubtedly examples of fallacious reasoning, in which the observed phenomena were ascribed to causes which did not produce them. They have generally been regarded as so many proofs of the influence of the imagination over the functions of the body; but though in many of them the morbid phenomena were probably modified in a considerable degree by the states of the patients' minds, there can be little doubt that in the great majority of them the recoveries were simply the results of the natural course of the disease.

We constantly meet with cases in which the imagination causes not only marked subjective, but likewise, though much more rarely, objective symptoms. Dr. H. Bennett, at page 289 of his work on the *Principles and Practice of Medicine*, relates a good illustration of the former:—'A butcher was brought into the shop of an apothecary in Edinburgh, from the market-place opposite, labouring under a terrible accident. The man, on trying to hook up a heavy piece of meat above his head, slipped, and the sharp hook penetrated his arm, so that he himself was suspended. On being examined, he was pale, almost pulseless, and expressed himself as suffering acute agony. The arm could not be moved without causing excessive pain, and in cutting off the sleeve he frequently cried out; yet, when the arm was exposed, it was found to be uninjured, the hook having only traversed the sleeve of his coat.'

To show how the imagination alone can produce objective symptoms, I may here give an account of trials made some years ago at the Hotel Dieu of Paris, under the superintendence of M. Trousseau, with pills composed of wheaten flour and a little gum arabic. The second case in which they were used was a girl affected with dyspnœa, which neither auscultation nor percussion could ascribe to any organic affection of the chest. From its intermittence and alternation with obscure pains, it was supposed to be purely nervous, or at most rheumatic. Three pills daily were ordered: milk diet.

'At nine o'clock A.M. first pill was taken. Patient afterwards felt a burning sensation at pit of stomach, precordial anxiety, followed by heat of skin and itching, great general excitement, which was relieved only after she had coughed up some bloody sputa. Perspiration then began, with an abundant discharge of urine. Second pill, taken at two o'clock P.M., produced same effects as former one, but they were more marked; same bloody sputa. Third taken at five o'clock P.M.: same effects; diuresis still more copious. During night, obliged to get up to pass water, though not in the habit of doing so.

'Next morning, before the visit, she took the fourth pill (contrary to orders), which produced the same phenomena as yesterday. When seen at our visit, the feverish heat was intense, itching of skin insupportable. Apprehensive lest her health might suffer from continuing the trial longer, it was stopped. Milk diet continued. Next day, patient felt well, with the exception of a little general soreness from yesterday's attack. On the following day, ordered four half-pills to be taken in twenty-four hours. The first half caused, like the other three taken at intervals of a few hours, heat over the whole body, irritation of the stomach, headache, slight perspiration, followed by calm sleep. Secretion of urine again increased, as in former trial, although the diet was the same as during the previous days.'

The trials were recommenced several times at short intervals, with similar results. They constantly caused general excitement, which seemed always to originate in the stomach. Increased perspiration, with itching of the skin, and increased diuresis, were the least variable of the symptoms. No more spitting of blood occurred, although the temporary increase of the dyspnœa seemed to indicate transient congestion of blood to the lungs. No further trials were made on this patient.

Every medical man is more or less familiar with the injurious effects caused on patients by depressing emotions, and with the favourable influence produced by those of a cheerful nature. Though the importance of these facts is understood and admitted, it appears to me that in practice it is too frequently overlooked. I have often enough had occasion to see serious cases of disease carefully examined and prescribed for by able and experienced physicians, without the slightest inquiry being made respecting the *morale* of the patients. Under such circumstances, if any depressing emotion was exhausting the energy of the patient's nervous system, it could not be expected that the results of the treatment would be satisfactory. I remember the case of a lady who had been treated for several months by a physician of great merit. Her health, sometimes better, sometimes worse, at last became decidedly worse; her sleep became impaired, her strength diminished, her appetite fell off, and considerable emaciation took place. Another physician, who had been requested to take charge of the case, accidentally ascertained that his patient was painfully preoccupied with the idea that she was suffering from a cancerous affection.

In a short time he succeeded in convincing her that her apprehensions were groundless; and in a few weeks the lady's health was satisfactorily restored, though no particular change had been made in the plan of treatment employed by the former physician.

In a severe attack of disease, occurring after a certain age, the patient is generally more or less anxious about how it will terminate; and in a nervous individual the anxiety may assume the form of a fixed conviction that he cannot recover, although, from the nature of the disease, such apprehension may be groundless. It happens frequently enough, however, that mental excitement produced by causes arising from the unavoidable influence of the patient's social relations, and which have nothing to do with the state of his health, exercise a most injurious influence on the course of disease. In all such cases, the removal or alleviation of whatever mental state depresses or exhausts the energy of the nervous system must be considered a point of very great importance; indeed, in many cases of functional disease in nervous individuals, the method of treatment must have reference more to the frame of the patient's mind than to the state of his body.

In cases of disease that will probably terminate fatally, it is the duty of the medical man to make the patient acquainted with his state, either directly, or what is perhaps better, through some other person. I think that grave announcement should never be made absolutely, and without more or less modification. However great a patient's strength of mind may be, there are few who would not be more or less depressed by such an announcement; and the most conscientious physician will find in the occasional fallacy of the most certain prognosis sufficient to justify him in not withholding from his patient the shadow at least of a hope. I think the simple announcement to a patient, that his case is utterly hopeless, would give a stronger shock to the nervous system than some such phrase as the following: 'As there is no hope for you from anything that medicine can do, you must prepare yourself for the worst. However, we now and then see the most desperate cases wonderfully prolonged, and even, though very rarely, restored to health.' But the circumstances of the case will modify the form of the phrase.

However well prepared a patient may fancy himself to be for calmly hearing the announcement of his fate, it frequently happens that the words, 'We can do nothing more for you,' produce a marked depression of the vital powers, already more or less impaired by disease. I remember well the case of a gentleman, on whose labours the comfort of many persons depended. He was affected with phthisis, but it was probable his life would be prolonged for many months. He urged his medical man to tell him frankly the nature of his illness—that he was quite prepared in case his complaint should be one he dreaded. After the announcement the patient's strength rapidly sank, and he died about three weeks afterwards.

In all important cases, therefore, I think a physician ought as habitually to pass in review the frame of his patient's mind as he does the state of his different organs, and to employ whatever means he may think most likely to diminish or to remove any depressing moral influence. It is evident that in such cases much will depend on the knowledge of human nature possessed by the physician, and on the tact and judgment with which he applies means suited to the circumstances of individual cases. Hope, some one has said, is the finest of stimulants; and the physician who can inspire his patients with confidence, or who can moderate or remove their depressing emotions, can accomplish results invaluable in all treatment.

I shall not advert to the interesting subject of hypnotic therapeutics, as my object is rather to point out the importance of attending to the state or frame of the patient's mind in disease, than to indicate any particular plan of treating a diseased state by acting through the mind on the body,

I shall now bring my work to a close by giving a summary of the principal conclusions which I think may justly be drawn from my investigation of the important subject of the treatment of disease:—

1*st*, That as yet we have no system of therapeutics based on rational or well-established principles.

2*d*, That our ignorance of the natural course of disease, or, in other words, of the curative resources of the organism, and of the curative properties of drugs, has been, and still is, the chief source of error in therapeutics, and the chief obstacle to its improvement.

3*d*, That, until it has been removed or lessened by proper investigations, the best general method of treatment is the expectant, or rather the so-called palliative, combined with judicious hygienic and dietetic means, except in the comparatively small number of diseased states for which we possess decidedly curative agents, as quinine in intermittent fever, etc.

THE END.

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